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THE
ANNALS
OF
PHILOSOPHY.

NEW SERIES.

JANUARY TO JUNE, 1821.



VOL. I.

OR THE SEVENTEENTH FROM THE COMMENCEMENT.

London:

Printed by C. Baldwin, New Bridge-street;

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* * *A few Copies of the first Series of the Annals of Philosophy may be had by application to the publishers, or to any bookseller. It consists of Sixteen Volumes, having commenced with the year 1813.*

ERRATA.

In the Number for July, 1820, page 48, line 10, from bottom, *for* evolving, *read* cooling.

In the Number for August, page 100, line 23, *for* Mullen glass; *read* Müller glass.

ERRATA IN No. XCIV. FOR OCTOBER, First Series.

Page 281, line 9, *for* June 30, *read* June 3.

283, line 3, *for* w l , *read* n l ,

4, *for* $(n - 1) x$, *read* $(n - 1) w$.

286, line 20, *for* $\cot. n x$, *read* $\cot. n w$.

287, line 5, *for* $\phi (x_n)$, *read* $\phi_n = (x_n)$.

ERRATA IN VOL. I. New Series.

Page 46, last line, *for* three inches, *read* eight inches.

for three inches from the bottom, *read* eight inches.

It escaped notice, until too late, that in both these instances, the printer had mistaken a figure of eight for three.

The dimensions, as corrected, are those of the boiler and fire-place used in some experiments made at Bromley; in the greater number of those alluded to by Dr. Bostock, the dimensions of the boiler and fire-place were as follow:

Boiler.—Three feet long, 15 inches wide, and 15 inches deep.

Fire-place.—Nineteen and a half inches long, nine inches and a half wide, and ten inches deep.

Page 57, line 31, *for* 30, *read* 25.

58, line 9, *for* one atom of deutoxide *read* four atoms.

89, line 35, *for* a , the joinings, *read* at the joinings.

36, *for* w , *read* w .

94, line 8, *for* flannel, *read* flame.

98, last line, *for* equations, *read* equation.

105, line 4, *for* N , *read* D .

$$7, \text{ for } + \frac{x^7}{1 \dots 7}, \text{ read } - \frac{x^7}{1 \dots 7}.$$

22, *for* C , *read* b .

136, line 6, *for* Mezthyr, *read* Merthyr.

34, *for* thin bitumen, *read* elastic bitumen.

160, line 5 from bottom, *for* Capricornus, *read* Sagittarius.

212, line 32, *for* burning, *read* liming.

218, line 34, *for* being change, *read* being no change.

38, *for* on chlorine gas, *read* in chlorine gas.

222, line 36, *for* pel de singette, *read* sel de Seignette.

7, from bottom, *for* distilled, *read* deducted.

274, line 3, from the top, *for* FRS. *read* VPRS.

276, line 11, from the bottom, *for* receive, *read* conceive.

287, line 18, *for* $(a - b) A$, *read* $(a - b) A$.

ADVERTISEMENT.

I PROJECTED the *Annals of Philosophy* in the year 1812, and have continued to be the Editor ever since, with the exception of a single year, when my sudden removal to Glasgow, and the necessity I was under of beginning a laborious course of Chemical Lectures with scarcely any previous preparation, and with no assistant, put it out of my power to devote a sufficient portion of my time to the laborious and diversified duties of Editor of a periodical work of science. My friends Dr. Bostock and Mr. Arthur Aikin were kind enough to supply my place during that year; and carried on the *Annals of Philosophy* with a spirit which left the readers no cause to regret the temporary absence of the original Editor. The principal object which I had constantly in view was to render the *Annals* as complete a register as possible of all the improvements made in chemistry and the kindred sciences, not merely in Great Britain, but in every part of the world. How far my exertions were attended with success, it is not for me to determine, though I can say with truth that neither labour nor expense was spared to fulfil, as far as was in my power, the objects which I had in view.

After a trial of two years, I have satisfied myself that a residence more than four hundred miles distant from the place of publication is scarcely consistent with the active duties of an Editor. It more than quadruples the labour, while it diminishes, almost in the same proportion, its successful exertion. I have, therefore, with the concurrence of the publishers, transferred the Editorship of the *Annals of Philosophy* to my friend Mr. Richard Phillips, a gentleman whose scientific talents and acquirements are too well known to require any observations on my part.

But though I relinquish the Editorship, I do not abandon all interest and concern in the work. I shall still continue connected with it, and, I trust, shall be a frequent and active contributor, though my absence from the place of publication renders the labours of the active Editorship irksome and painful.

In consequence of this change, it has been determined to denominate the seventeenth volume of the *Annals of Philosophy* the FIRST of a NEW SERIES.

Glasgow,
December, 1820.

THOMAS THOMSON.

I undertake the Editorship of the Annals of Philosophy, announced in the above advertisement, with some anxiety, lest the reputation which the work has acquired should be diminished by being placed under my controul. The friendly assurances of Dr. Thomson, that he will continue to contribute to the Annals, will, I trust, convince the reader that the work will in future possess a great share of its present value.

The successful management of a scientific journal is rendered doubly difficult by the numerous similar sources through which information is now conveyed: relying, however, upon the favourable circumstances in which I am placed for the early acquisition of philosophical intelligence, I trust I shall be enabled, with the assistance of which I have received numerous promises, to render the work not unworthy of the patronage and support of the scientific public.

London,
December, 1820.

RICHARD PHILLIPS.

ANNALS

OF

PHILOSOPHY.

JANUARY, 1821.

ARTICLE I.

Experiments to determine the true Atomic Weight of Strontian, Lime, Magnesia, Phosphoric Acid, and Arsenic Acid. By Thomas Thomson, M.D. F.R.S.

IN two papers published in the preceding volume of the *Annals of Philosophy*, I have endeavoured to determine the real weight of the atom of 13 simple bodies, and a considerable number of compounds formed by the union of these bodies with each other. In every one of these cases we found reason to conclude that the atomic weight of every body is a multiple of the weight of an atom of hydrogen. I showed that the determinations of Dr. Wollaston and Prof. Berzelius, though in most cases constituting very good approximations, are never rigidly exact, the true weights of the atoms of bodies constituting much simpler numbers than they have supposed. I have shown, I trust to the satisfaction of all practical chemists, that the mode of experimenting adopted by Berzelius is not precise enough for the determination of the weights of the atoms of bodies. I ought to observe, however, that Berzelius is undoubtedly a very great master of the analytical art; that his analyses approach upon the whole exceedingly near the truth; and exhibit a consistency which is highly satisfactory, and does a great deal of credit to the industry and sagacity of their author. But, unfortunately his mode of experimenting admits of no criterion by which the experimenter can determine whether the results be accurate or not; so that he has no means of checking himself, and of taking

those precautions which a knowledge of the existence of error would lead him to look for.

The mode of experimenting which I have employed has the great advantage of informing the experimenter whether or not his results be accurate. When I mix together a given weight of sulphate of potash and nitrate of lead, it is easy, by examining the clear liquid which remains after the precipitate has subsided, to ascertain whether that liquid holds in solution any sulphuric acid or any oxide; thus showing whether either of the salts has been employed in excess, and which of the two. We are thereby enabled to vary the weight of each salt till at last we hit upon proportions which exactly decompose each other without leaving any excess whatever. The knowledge of these proportions enables us, by the methods explained in the papers above alluded to, to determine the real weight of the atom of the bodies which are the subject of experiment.

This method is tedious, and requires no great share of dexterity in experimenting. We must take care that our salts are pure and dry; that they are accurately weighed out, completely dissolved in distilled water, and the solutions well mixed without any loss whatever. Whoever attends to these precautions will be enabled to come to results as near absolute accuracy as is consistent with experimenting. I have made considerable progress in determining the weights of the atoms of the acids and bases by this method; but at present I mean to confine myself to the experiments made to determine the atomic weights of the five substances whose names are placed at the head of this essay. The following table exhibits the numbers assigned to the atom of these bodies by Dalton, Wollaston, Berzelius, Prout, and myself:

	Dalton.	Wollaston.	Berzelius.	Prout.	Thomson.
Strontian.	6.571	6.9	12.9460	—	6.5
Lime.	3.428	3.546	7.1206	3.5	3.625
Magnesia.	2.428	2.46	5.1672	—	2.5
Phosphoric acid. . .	3.285	3.74	8.9230	3.75	3.5*
Arsenic acid.	—	—	14.4077	—	7.25

To enable the reader to compare Berzelius's atoms with the others contained in the table, it will be proper to bring them down to smaller numbers. This may be done by dividing each by 2. They will then be reduced to what they are in the following table:

* In the fifth edition of my *System of Chemistry*, I made it 4.5, deceived by the experiments of Berzelius and Dalton. But I have long ago corrected this mistake.

Strontian	6·4730
Lime	3·5603
Magnesia	2·5836
Phosphoric acid	4·4615
Arsenic acid.	7·20385

When thus stated, they approach very nearly to the other atomic numbers contained in the table, though the number of decimal places is sufficient to render their absolute accuracy suspicious. They want one of the criterions which I have shown, in my preceding papers, that the real atomic weights of bodies possess. They are none of them multiples of 0·125, the weight of an atom of hydrogen. Dalton's numbers were obtained by dividing the atomic weights which he gives in his *System of Chemical Philosophy* by 7, which is his weight of an atom of oxygen. This is a fairer method than the one which I employed in my last paper. I there took in his error with respect to the weight of oxygen when compared with hydrogen. Here that error is not reckoned at all, but tacitly corrected.

Let us now have recourse to experiments to determine which of these columns comes nearest the truth, or whether any of them be absolutely correct. Sulphate of soda is composed as follows :

Sulphuric acid	5
Soda	4
	<hr/>
Sulphate of soda	9

According to the numbers which I have made choice of for chlorine and strontium, chloride of strontium is composed of

Chlorine	4·5
Strontium	5·5
	<hr/>
Chloride of strontium	10·0

To determine whether these numbers be correct, I took nine grains of sulphate of soda recently kept for half an hour in the state of igneous fusion, and dissolved it in a small quantity of distilled water. I likewise dissolved in another portion of distilled water 10 grs. of chloride of strontium, recently fused and perfectly dry. These two liquids were carefully mixed together, and allowed to remain in contact for 24 hours. I then drew off a portion of the clear liquid, and examined it. No change whatever was produced in it by nitrate of barytes. Of course, it contained no sensible quantity of sulphuric acid. It was not rendered in the least turbid by sulphate of soda, phosphate of soda, arseniate of soda, or chromate of potash. Hence there is no evidence that it contained any strontium.

I consider myself as warranted by the preceding experiment to conclude, that chloride of strontium is composed of

Chlorine.	4.5
Strontium.	5.5
	<hr/> 10.0

Consequently muriate of strontian is composed of

Muriatic acid	4.625
Strontian	6.5
	<hr/> 11.125

It is obvious, therefore, that the true weight of an atom of strontian is 6.5, the very weight which I have already assigned. The above experiment will neither succeed with Dalton's, Wollaston's, nor Berzelius's numbers—a sufficient proof that none of them is absolutely correct.

2. I made a great many trials before I was able to determine the weight of an atom of lime; but I consider it as needless to relate those which were unsuccessful. Indeed I failed so frequently that I for some time despaired of being able to determine the point. However by persevering and trying one method after another, I at last hit upon a way which is quite easy, and which, if carefully performed, yields most satisfactory results. No salt of lime which I tried possessed sufficient solubility, and the capacity of being totally deprived of water by heat without decomposition. I was obliged, therefore, to have recourse to calcareous spar. I took perfectly pure calcareous spar, reduced it to a coarse powder, and exposed it for an hour to a temperature of between 300° and 400°, which renders it perfectly dry without disengaging any sensible quantity of carbonic acid. It will appear by the experiments which I am going to relate, that pure dry carbonate of lime is composed of

Carbonic acid.	2.75
Lime.	3.5
	<hr/> 6.25
Carbonate of lime	6.25

When pure crystals of bicarbonate of potash are exposed to a red heat in a crucible of platinum, one half of the carbonic acid is driven off, and there remains pure and dry carbonate of potash. Now this salt is composed of

Carbonic acid.	2.75
Potash.	6.00
	<hr/> 8.75
Carbonate of potash	8.75

The knowledge of the composition of these salts will enable the reader easily to follow the experiments which I am going to relate.

6.25 grs. of dry calcareous spar were dissolved in muriatic

acid, and the solution was evaporated to dryness on a sand-bath at a temperature not much exceeding 100° . The dry salt was then dissolved in distilled water. It obviously contained 3.5 grs. of lime saturated with muriatic acid; and from the result of the experiment, it will immediately appear that muriate of lime is a compound of

Muriatic acid.	4.625
Lime	3.5
Muriate of lime.	<u>8.125</u>

8.75 grs. of carbonate of potash were dissolved in a separate portion of distilled water, and the solution was mixed with that of the muriate of lime. A double decomposition took place, carbonate of lime precipitated, and the clear liquid held muriate of potash in solution. A portion of this liquid was drawn off and examined. It produced no change upon the colour of paper stained red by cudbear.* Oxalate of ammonia occasioned no precipitate in it. From these facts I consider myself entitled to conclude that the whole of the carbonate of potash was exactly neutralized by muriatic acid, and that the whole of the lime had been precipitated in the state of a carbonate. It obviously follows that the weight of an atom of lime is 3.5. This is the atomic weight already assigned by Dr. Prout. My number is too high, and Mr. Dalton's too low. The number of Wollaston and Berzelius is much nearer the truth than either Dalton's or mine, though both of them are rather too high.

To enable the reader to judge of the accuracy of the preceding conclusion, it may not be amiss to select two out of the great number of experiments which I made with a view to determine the weight of an atom of lime.

(1.) If we suppose the atom of lime to weigh 3.625, which is the atomic weight that I had pitched upon, then it is clear that dry carbonate of lime must be composed of

Carbonic acid	2.75
Lime	3.625
Carbonate of lime.	<u>6.375</u>

To verify this supposition, I dissolved 6.375 grs. of dry carbonate of lime in muriatic acid, evaporated the solution to dryness, redissolved the residue in distilled water, and mixed it with a solution of 8.75 grs. of carbonate of potash. After the carbonate of lime had precipitated, a portion of the clear liquid was drawn off, and some oxalate of ammonia dropped into it. An abundant precipitate of oxalate of lime fell down. Hence

* This is the most delicate test of alkalis that I am acquainted with. A very minute portion of alkali or alkaline carbonate renders it violet coloured.

lime still remained in solution; consequently the carbonate of potash had not been sufficient to throw down all the lime. We see from this that an atom of lime is not so heavy as 3.625.

(2.) The nearest multiple of 0.125 to Mr. Dalton's atomic weight of lime is 3.375. Now if this be the weight of an atom of lime, it is plain that dry carbonate of lime must be composed of

Carbonic acid	2.75
Lime	3.375
Carbonate of lime.	<u>6.125</u>

To verify this supposition, I dissolved 6.125 grs. of carbonate of lime in muriatic acid, and after treating the solution in the manner already described, I mixed it with a solution of 8.75 grs. of carbonate of potash. After the carbonate of lime had subsided, the clear supernatant liquid was not precipitated by oxalate of ammonia, and, therefore, contained no lime; but it instantly rendered cudbear paper violet, and therefore contained an excess of potash. Hence the muriatic acid united with the lime had not been sufficient to saturate the whole of the potash. It is clear from this that an atom of lime is heavier than 3.375.

These experiments are sufficient I trust to satisfy the reader that the true weight of an atom of lime is 3.5.

3. I found much less difficulty in determining the weight of an atom of magnesia than of lime. Sulphate of magnesia may be exposed to a red heat, and rendered thoroughly dry without losing any of its acid, if the experiment be properly conducted. If the weight of an atom of magnesia, which I have assigned, namely, 2.5 be correct, it is obvious that anhydrous sulphate of magnesia is composed of

Sulphuric acid	5.0
Magnesia	2.5
Sulphate of magnesia	<u>7.5</u>

Anhydrous chloride of barium, as I demonstrated in a former paper, is composed of

Chlorine	4.5
Barium	8.75
Chloride of barium ..	<u>13.25</u>

7.5 grs. of anhydrous sulphate of magnesia, and 13.25 grs. of chloride of barium, were respectively dissolved in two distinct portions of distilled water, and the solutions mixed together, and well agitated. After the sulphate of barytes had subsided, a portion of the clear liquid was drawn off and examined: neither sulphate of soda nor nitrate of barytes occasioned any precipitate or muddiness in it. Hence it is obvious that the liquid neither

contained any barytes nor any sulphuric acid in solution; so that the sulphuric acid in the 7.5 grs. of sulphate of magnesia had just saturated the barytes from 13.25 grs. of chloride of barium. It is obvious then that the true weight of an atom of magnesia is 2.5; therefore, the atomic weight assigned by Dalton and Wollaston is too small, while that assigned by Berzelius is too high. Accordingly, if we mix together anhydrous sulphate of magnesia and chloride of barium in the proportions indicated by these numbers, we shall in the one case find an excess of sulphuric acid, and in the other of barytes, in the liquid after the precipitate has subsided, indicating obviously an error in the weight of the salts thus mixed together, and consequently an error in the numbers assigned by these gentlemen for the weight of an atom of magnesia.

4. The weight of an atom of phosphoric acid has cost me first and last a good deal of trouble. I have the happiness, however, at last to be able to lay before the reader experiments of so decisive a nature that no doubt nor uncertainty can rest upon the subject for the future. In the year 1816 I drew up a paper upon the subject, the result of a good many experiments, which was read before the Royal Society. Some discussion took place in the committee of papers relative to these experiments; and Dr. Wollaston, who was a member of that committee, and to whose friendship and assistance I have been very frequently obliged, kindly brought the paper to me to give me an opportunity of correcting some numerical mistakes which he had observed in it. By this time I had made the experiments on phosphuretted hydrogen gas, which were soon after published in the *Annals of Philosophy*. These experiments had made me acquainted with the true weight of the atom of phosphorus, phosphorous acid, and phosphoric acid, and had explained all the errors into which I had fallen in my original paper. I had, therefore, been extremely desirous of withdrawing my paper from the Royal Society, in order to have an opportunity of correcting it. Of course, when it was put into my hands by Dr. Wollaston, I requested of the Society to be allowed to keep it, and this request they were kind enough to indulge me in.

Just at the time that my proof sheet giving an account of phosphorus in the fifth edition of my *System of Chemistry* was in my possession, I received Mr. Dalton's short paper on phosphuretted hydrogen gas, which was printed in the *Annals of Philosophy*. In that paper Mr. Dalton states that phosphuretted hydrogen gas is capable of condensing twice its volume of oxygen gas. I had just before been informed by Gay-Lussac of Dulong's discovery of hypophosphorous acid, and had read over Berzelius's paper on phosphorus and its compounds, in which he shows by a number of analyses agreeing very well with each other that the atomic weight of phosphoric acid is 4.5, or at least very near that number. Being perfectly sure of the accu-

racy of my own experiments, and not suspecting any inaccuracy in Mr. Dalton's, I naturally concluded that phosphuretted hydrogen gas was capable of uniting with three proportions of oxygen gas; namely,

1 volume phosphuretted hydrogen with	1.0 volume oxygen
1	1.5
1	2.0

The first two of these proportions were my own; the last was Dalton's. Now phosphuretted hydrogen gas is composed of

1 volume hydrogen gas	} condensed into one volume.
1 volume phosphorous vapour	

The volume of hydrogen gas requires half a volume of oxygen gas to convert it into water. Therefore, we have

1 volume phosphorus uniting with	0.5 volume oxygen
1	1.0
1	1.5

This is the same thing as saying that an atom of phosphorus unites with one atom, two atoms, and three atoms of oxygen. I had demonstrated that an atom of phosphorus weighs 1.5. Hence it was obvious that the weights of these three compounds of oxygen and phosphorus must be as follows:

First compound.....	2.5
Second ditto.....	3.5
Third ditto.....	4.5

I concluded that these three compounds were hypophosphorous acid, phosphorous acid, and phosphoric acid. This reconciled my own experiments with those of Dalton and Berzelius. I was induced by this reasoning, which will be admitted to be sufficiently plausible, to alter the proof sheet, and to bring it to the state in which it appeared in the fifth edition of my *System of Chemistry*.

Soon after this I went to Glasgow, and nearly a year elapsed before I was in possession of a laboratory, or had it in my power to return to the subject. As soon as I had the means I tried Mr. Dalton's experiment, and was a good deal surprised to find it inaccurate. I was unable to obtain a complete combustion of a mixture of one volume phosphuretted hydrogen gas and two volumes of oxygen. I was now satisfied that my original views on the subject were correct; and Davy's paper on the subject, which appeared about this time, and which agreed exactly with my original views, served to confirm my opinion. Berzelius's experiments still remained to be accounted for. I was satisfied that they must be inaccurate; but the difficulty was to hit upon a method of demonstrating them to be so. The experi-

ments which I am now going to relate will leave no doubts on the subject in the mind of any practical chemist. I do not know how Berzelius has deceived himself; but that he laboured under some deception or other will not admit of a doubt.

It would be tiresome and perfectly unnecessary to relate the numerous experiments which I made to determine the weight of an atom of phosphoric acid. I shall confine myself to those which answered the object that I had in view.

If Berzelius's opinion respecting the weight of an atom of phosphoric acid be true, then anhydrous phosphate of soda is composed of

Phosphoric acid	4.5
Soda	4.0
Phosphate of soda	8.5

We have seen in a former paper that dry nitrate of lead is composed of

Nitric acid.	6.75
Protoxide of lead	14.00
Nitrate of lead	20.75

To put Berzelius's analyses to the test of experiment, I exposed a quantity of pure crystallized phosphate of soda to a red heat, and kept it in fusion for half an hour. 8.5 grs. of this anhydrous salt and 20.75 grs. of dry nitrate of lead were respectively dissolved in separate portions of distilled water, and the solutions were intimately mixed together. After the phosphate of lead had precipitated, a portion of the clear liquid was drawn off and examined. It yielded a white precipitate when mixed with nitrate of lead and with muriate of lime. Hence it obviously contained phosphoric acid in solution. Hence the oxide of lead in 20.75 grs. of nitrate of lead is not capable of neutralising all the phosphoric acid in 8.5 grs. of anhydrous phosphate of soda. It follows from this that Berzelius's statement of the composition of phosphate of soda is incorrect.

If my own opinion respecting the composition of phosphoric acid be true, its weight must be 3.5, and anhydrous phosphate of soda must be composed of

Phosphoric acid	3.5
Soda	4.0
Phosphate of soda	7.5

To verify this opinion, 7.5 grs. of anhydrous phosphate of soda and 20.75 grs. of dry nitrate of lead were dissolved respectively in separate portions of distilled water, and the solutions mixed together. After the phosphate of lead had subsided, a

portion of the clear liquid was drawn off and examined. It yielded no precipitate when mixed with nitrate of lead or muriate of lime; and, therefore, contained no phosphoric acid in solution; neither was it affected when sulphate of soda was dropped into it—a proof that it was equally free from lead. We see then that the phosphoric acid in 7.5 grs. of anhydrous phosphate of soda is exactly neutralized by the oxide of lead in 20.75 grs. of nitrate of lead. Therefore an atom of phosphoric acid weighs 3.5, and phosphate of lead is composed of

Phosphoric acid ..	3.5	20	100
Oxide of lead.	14.0	80	400
			<u>17.5</u>		<u>100</u>

Here then is the source of Berzelius's mistakes. He makes the composition of phosphate of lead to be

Phosphoric acid	100
Protoxide of lead.	314

I do not know the reason of this difference. It must, I think, be owing either to a mixture of two phosphates of lead, or to the formation of a compound different from neutral phosphate of lead. I obtained a result approaching to that of Berzelius, when I attempted to determine the composition of phosphate of lead by direct experiments. But be the cause of the error what it may, there can be no doubt entertained of its existence, at least by any person who will take the trouble to repeat the experiment which I have just described.

We see that the weights of the atom of phosphoric acid and of lime are exactly equal. Hence neutral phosphate of lime is composed of

Phosphoric acid.	50
Lime	50
	<u>100</u>

When I first attempted to determine the weight of an atom of phosphoric acid, I had recourse to salts of lime, knowing the perfect insolubility of phosphate of lime in water. I dissolved determinate weights of carbonate of lime in muriatic acid, evaporated the solution to dryness, and mixed it with a determinate weight of phosphate of soda; but I soon found that it is impossible to precipitate lime completely from a muriatic acid solution of lime (though perfectly neutral) by means of phosphate of soda. The solution of muriate of soda has the property of holding phosphate of lime in solution, and the presence of lime is always indicated in the clear solution by means of oxalate of ammonia, which throws down a copious precipitate of oxalate of lime. I have no doubt that this solubility of phosphate of lime in solu-

tions of muriate of soda, and probably in many other saline solutions, has misled experimenters in their attempts to analyze the phosphates.

I intend the first leisure opportunity to correct my old paper on phosphoric acid, and give it to the public. It contains many facts still unknown to chemists in general, notwithstanding the experiments of Berzelius on the subject; and now that I am aware of what the true composition of the phosphates is, it will not be so difficult to obtain accurate results.

5. The exact knowledge of the weight of an atom of arsenic acid is of considerable importance towards the perfection of the atomic theory. Hitherto Berzelius is almost the only person who has made direct experiments to determine the atomic weights of arsenious and arsenic acids. He has concluded from his experiments, and the conclusion seems to have been acquiesced in by chemists in general, that the oxygen in arsenious and arsenic acids are to each other in the ratio of 3 to 5. It will be seen from the table near the beginning of this paper, that my atomic number for the weight of an atom of arsenic acid very nearly agrees with that of Berzelius. In a paper published in a late volume of the *Annals of Philosophy*, indeed, I endeavoured to show that the double of my number, or 14.5, which very nearly agrees with the number actually pitched on by Berzelius, is the real weight of the atom of arsenic acid. The object which I had in view was to get rid of certain fractions which disfigure the composition of arsenious and arsenic acids as I represented them in the fifth edition of my *System of Chemistry*. The experiments which I am now going to relate will show how far these views are consistent with matter of fact.

If arseniate of soda in crystals be, as I represented it in a preceding paper, a compound of 1 atom arsenic acid = 14.5 and 1 atom soda = 4, then its composition must be as follows :

Arsenic acid	14.5
Soda.	4.0
<hr/>	
Arseniate of soda	18.5

We have seen already that nitrate of lead is composed of

Nitric acid	6.75
Protoxide of lead	14.00
<hr/>	
Nitrate of lead	20.75

18.5 grs. of arseniate of soda previously kept in a state of igneous fusion for half an hour in a platinum crucible, were dissolved in distilled water. 20.75 grs. of nitrate of lead were dissolved in another portion of distilled water, and the two solutions mixed intimately with each other. After the arseniate of lead had subsided, the clear liquid was drawn off and examined. When mixed with nitrate of lead, a very copious precipitate fell,

showing that the liquid still contained a great deal of arsenic acid; consequently the supposition that the crystallizable arseniate of soda is a compound of one atom arsenic acid and one atom soda cannot be well founded. This will appear still more clearly by the following experiment:

Supposing that the crystallized arseniate of soda contained two atoms of arsenic acid united to one atom of soda, I took a quantity of carbonate of lime, equivalent to two atoms of lime, which is obviously 12.5 grs.; for carbonate of lime is composed of

Carbonic acid	2.75
Lime.	3.50
Carbonate of lime	<u>6.25</u>

And $6.25 \times 2 = 12.5$. This quantity I dissolved in muriatic acid, evaporated the solution to dryness, and dissolved the dry salt in distilled water. 18.5 grs. of dry arseniate of soda were dissolved in another portion of distilled water. These two liquids were mixed together. I was surprised to find that no precipitate of arseniate of lime, or at least only a very slight one, appeared. The solution had the property of reddening vegetable blues. We see from this, that binarsenate of soda is incapable of decomposing muriate of lime. When ammonia was poured into the solution, a copious precipitate of arseniate of lime fell in small silky needles. After the precipitate had subsided, the clear liquid was found to precipitate, when mixed with oxalate of ammonia. It, therefore, contained an excess of lime. This experiment shows clearly that the crystallized arseniate of soda is a binarsenate, and that an atom of arsenic acid weighs more than 7.25, the weight which I assigned in the fifth edition of my System of Chemistry.

After a great many trials, which I consider it as useless to relate, I found that 19.5 grs. of binarsenate of soda and 41.5 grs. of nitrate of lead, when separately dissolved in distilled water, and the solutions well mixed together, after all the arseniate of lead had precipitated, left a clear liquid which contained no sensible quantity of oxide of lead, or of arsenic acid. But if 18.5 grs. or 19 grs. of binarsenate of soda were employed, an excess of lead always remained in solution. From this experiment, it is obvious that the equivalent number for anhydrous binarsenate of soda is 19.5, and that it is a compound of 2 atoms arsenic acid = 15.5, and 1 atom soda = 4.

Hence an atom of arsenic acid, instead of weighing 7.25, as I supposed, weighs in fact 7.75, and arseniate of lead is a compound of

Arsenic acid	7.75
Protoxide of lead	14.00
Arseniate of lead	<u>21.75</u>

There is reason to conclude from the experiments made by different chemists on arsenic and arsenious acid, that the weight of an atom of arsenic is 4.75. It may be seen by consulting my *System of Chemistry* (either fifth or sixth edition) that Berzelius's experiments lead to the conclusion that arsenious acid is a compound of 4.75 arsenic + 1.5 oxygen. The preceding experiment leaves no doubt that arsenic acid is a compound of 4.75 arsenic + 3 oxygen. Thus we have these two acids composed as follows :

Arsenious acid of. . .	4.75 arsenic + 1.5 oxygen
Arsenic acid of.	4.75 + 3.0

We see that the oxygen in these two acids has not the ratio of 3 to 5, as Berzelius supposed, but of 1 to 2. The anomaly of $1\frac{1}{2}$ atom of oxygen combined with 1 atom of arsenic in arsenious acid still continues. I have not yet hit upon a method of putting the atomic weight of an atom of arsenious acid to the test of an unequivocal experiment ; but I have little doubt that the true weight of oxygen in arsenious acid combined with 4.75 arsenic is 2 ; and that the oxygen in arsenious and arsenic acids have to each other the ratio of 2 to 3 ; as is the case in sulphurous and sulphuric acids. I conceive the atomic weights of arsenic, arsenious, and arsenic acids, to be as follows :

	Weight of atom.
Arsenic	4.75
Arsenious acid	6.75
Arsenic acid	7.75

The two acids are composed as follows :

Arsenious acid	1 atom arsenic + 2 atoms oxygen
Arsenic acid ..	1 + 3

I do not despair of being able to decide this very important point hereafter by satisfactory experiments. In the mean time we may conclude that the ratio of 2 to 5, which Berzelius has endeavoured to establish in the oxygen combining with phosphorus and with arsenic, does not exist. This is a simplification of the atomic theory of some importance. I am thoroughly persuaded that as we proceed in our investigations, the simplicity of the atomic theory will become more and more apparent. The complex numbers of Berzelius will all disappear ; and the application of mathematical reasoning will by and by enable us to advance with unexpected rapidity in the chemical investigation of the vegetable and animal kingdoms.

ARTICLE II.

On the Geology of the Malvern Hills. By William Phillips, FLS. MGS L & C. and Hon. Mem. of the Cambridge Philosophical Society.

THE Malvern range of hills is peculiarly interesting as being surrounded by deposits which appear to have little geological connexion with its rocks, as well as from the remarkable composition of the latter. In the first volume of the Transactions of the Geological Society, there is an interesting communication on the subject, by Leonard Horner, Esq. FRS. &c.; but having, as I imagine, observed some circumstances deserving of note which escaped the notice of that gentleman, I venture to suppose them worthy of a place in the *Annals of Philosophy*; and as it would be difficult to render these observations intelligible without giving a general account of the range, I am induced to incorporate them with extracts from the paper above-mentioned, distinguishing such extracts by placing them between inverted commas; thus affording to the traveller a comprehensive view of this singularly interesting range, but premising that he will find, in the first volume of the Transactions of the Geological Society, many minutiae which do not appear essential to the present object, which is primarily to show that the rocks of this range are occasionally stratified; and secondly, to do away the too commonly received and erroneous opinion that they partly consist of granite.

"The Malvern hills are situated in the south-western part of Worcestershire; the boundary which divides the counties of Worcester and Hereford passes along their western side. They consist of an uninterrupted chain of about nine miles in length from north to south, their greatest breadth not exceeding two miles. The several parts of the chain present roundish summits," but the nearly continuous line formed by the summit of the central part of the range forms a remarkably narrow ridge, which, except here and there, is, in common with the rest of these hills, covered with short grass and moss: fern is also seen on the sides, except where the rocks rise from beneath the vegetation.

"The highest point of the range is the Worcestershire Beacon, which is 1444 feet above the level of the sea; the Herefordshire Beacon, and North Hill, are somewhat lower."

The range is flanked on the western side by limestone in remarkably regular strata dipping generally to the north or north-west, at a low angle, and on the east, and the north and south extremities, by the New Red Sandstone or Red Marle, which is visible beside the road in two or three places on the south of Great Malvern, at a higher elevation than that place,

and at the nearest to it in strata dipping to the east conformable with the dip of the hill, it is, therefore, very considerably higher than the country on the east of the range which consists of the same deposit.

The soil which supports the moss and grass with which these hills are generally covered appears, for a few inches in depth, to assume the character of vegetable mould, but that there is below it, and covering the rocks of which the hills are constituted, a depth of loose earth, is not only evinced by the generally smooth surface of the hills, and by the occasional openings beneath the vegetation, but also by the fresh mole-hills which are found in many places, and even near the summits of some parts of the range. This loose earth varies in colour from that of common sand to a fawn colour, and in substance sometimes resembles sandy loam in appearance: it is probable that it has resulted from the decomposition of the softer and more readily decomposable rocks of these hills, and it is owing to this decomposition that "the comparatively little opportunity for examining the nature of the rocks of this range is confined to those which rise here and there on the summits and sides above the grassy covering, and to the sides of the carriage road which runs near its base for a considerable distance on both sides the range, and round its northern termination, and to the quarries at the latter place, and also Castle Morton quarry, about two miles south of Little Malvern;" to which may be added the loose masses in the ravine behind Great Malvern, and in that between North Hill and End Hill.

It must be acknowledged that it is impossible to give to the rocks of these hills one general designation; but they appear to belong to sienite and the trap formation. The difference between sienite and greenstone consists only in the colour of the felspar. These rocks may, for the most part, be denominated greenstone; often, however, they consist decidedly of sienite, and sometimes may properly be termed sienitic greenstone; occasionally epidotic sienite. Whenever the felspar appears in any considerable mass, either in the form of a bed, or stratum, or of a vein, it is almost uniformly of a red colour, while in those in which that substance is in small grains (and is sometimes so small that the rock appears homogeneous), it is most commonly white, or of a greyish-white colour, more rarely red.

The minerals of this range may be considered as being comprehended in the following list:

Crystalline hornblende may be considered as being the prevailing rock of these hills.

Red felspar, often enclosing hornblende, sometimes mica, epidote, or calcareous spar; occasionally interstratified, and in veins.

An *earthy substance*, sometimes resembling lithomarge, but

which appears to be hornblende in a peculiar state, probably in that of decomposition.

Mica in veins and beds, enclosed in felspar, &c.

Talc, enclosed in felspar, &c.

Epidote, both compact and crystallized in veins, and occasionally forming an integrant part of the rock.

Quartz, in veins, and imbedded in felspar.

Heavy spar, in veins, &c.

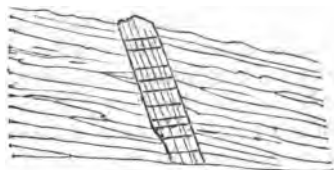
Magnetic iron ore.

The rocks of this range are for the most part, where they can be seen above the surface, "*confusedly heaped together*;" so that, except in two places, which will presently be noticed more particularly, no decisive appearances of stratification are to be observed, unless indeed we may be allowed to infer the existence of stratification wheresoever the hornblende rock assumes a slaty structure, indicating the direction of the dip; if this inference be allowable, it may be assumed that stratification is more common to these rocks than it appears at first sight.

The nearest place to Great Malvern at which a strong tendency to regular stratification appears, is about three miles on the south of it, and about 100 yards beyond the stone which is inscribed "*Ledbury, four miles.*" The beds here consist chiefly of red felspar, from an inch or less to a foot in thickness, enclosing quartz and hornblende, rarely mica: the interstratified substances are, hornblende, occasionally mixed with talc, and sometimes including a thin layer of red felspar and quartz, or of granular felspar with hornblende greatly resembling mica, of which the slaty structure is parallel with the beds of felspar: here and there are layers of granular quartz, mica, felspar, and hornblende, the plates of the mica being parallel with the general dip of the beds; hornblende, sometimes of a slaty structure, occasionally occurs in the same direction, but it now and then appears to pass into an earthy substance having somewhat the appearance of lithomarge. These beds vary from half an inch to a foot in thickness, and though not stratified with perfect regularity, are visible for nearly 100 feet in length, and 12 or 14 feet in height, above which the hill is covered with verdure. Many of the beds may be traced for several feet. These beds dip at an angle of about 20 degrees to the north-east.

In this place, these beds are traversed by a dyke of greenstone, about two feet wide, except that it narrows a little about 18 inches above the road, and dipping nearly north, at an angle of about 70° with the horizon. The top of this dyke protrudes above the beds it traverses, as is represented in the following sketch, but it was not to be perceived that these beds were at all disarranged by it, the portions of them next to it not being turned either upwards or downwards; nor did they seem to have suffered any alteration of texture or appearance where the contact was complete. The greenstone of the dyke is so remark-

ably fine-grained as to require the assistance of a glass to discover that it is a granular rock, and it lies in narrow layers running nearly parallel with the sides of the dyke, but which are traversed by crevices not quite at right angles, so as to divide the layers into quadrangular masses, which, though hard, are so small and brittle as scarcely to afford a surface of a square inch from a blow by the hammer. This appeared to be the only instance of a true dyke or vein among the rocks of the range.



Pursuing the road from Great Malvern towards Ledbury, the rocks on its side continue to present some, though less decided marks of stratification, until the road turns nearly due west; and just before it has attained the summit of the rise, a quarry appears on the top of the hill on the right, perhaps 100 feet above the road. This quarry is open to the south, and here stratification is obvious from below. On ascending it, the appearance is confirmed; stratification appears with nearly the same dip, but with much greater regularity than is apparent in the beds near the four-mile stone. The quarry is opened for upwards of 100 feet in length, and 40 feet in height; and several beds of the red felspar, which is the prevailing substance, may be traced very little short of the whole length, many of them upwards of 40 feet, dependent on the fall on the sides of the hill. The beds of felspar are thinnest near the summit; and the interstratified substances resemble those of the preceding instance, with some exceptions. Some of the upper beds present interstratifications of felspar, hornblende, and mica (?) and enclose small masses of attractive iron; others of slaty hornblende mingled with quartz; others again of felspar and hornblende. One stratum, above three inches in thickness, consisting wholly of slaty hornblende greatly resembling mica, may be traced for about 40 feet in length along nearly the middle of the quarry.

In the front of the quarry, and so detached as to allow a passage behind them, whence the rock had been taken away, stood two enormous blocks, each not less than 20 feet in height, and 10 feet in other directions, of slaty hornblende, of which the schistose structure was parallel with the dip of the regular beds, which the summits of these blocks still supported. The upper parts of these masses, where their structure was most regular, contained thin layers of red felspar enclosing quartz; these layers nearer the centre were less regular, and near the

bottom were quite irregular: the hornblende here assumed a more crystalline structure; and here, if not in most other places, the red felspar is not crystalline, but either compact, or granular.

In several places on the eastern side of the range, and particularly within a mile south of Great Malvern, many of the rocks in which hornblende greatly prevails, have that schistose structure which has been mentioned as being parallel with the beds above described: here, however, if this structure is to be considered as indicative of the direction of the strata, they will, for the most part, be nearly perpendicular to the horizon, but mostly with a slight inclination towards the north. In no other place do marks so indicative of stratification appear.

The Wych affords an excellent opportunity of viewing the rocks of that part of the range: it exhibits a complete jumble of most of the rocks discovered in it, not without some appearances of stratification, which by due examination prove to be fallacious.

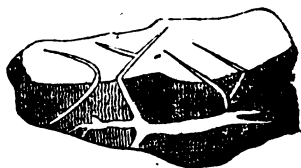
“Granite, rarely presenting the same appearance as that of Alpine countries,—not decidedly crystalline,—in which sometimes the quartz, sometimes the mica, is wanting,” has been described as being the prevailing rock at the Wych, as constituting a great part of End Hill, and the upper part of North Hill, and Swinnit Hill; but it is also acknowledged that “the mere term granite would convey to most mineralogists an erroneous idea of the nature of these rocks.” An anxious search among these rocks every where for more than three parts of the way along them southwards from their termination on the north, did not satisfy me that even a single hand-specimen of well-characterized granite could be found. Granite is commonly understood to be a rock, in which its ingredients, quartz, felspar, and mica, are all decidedly crystalline, without the appearance of one of them as an imbedding substance. In the “granite” of this range, the felspar is invariably an imbedding substance, and compared with hornblende, is rarely the imbedded substance; it may be said rarely to contain either quartz or mica, although each is sometimes well defined, but never, as far as my observation goes, is unaccompanied by hornblende. Hornblende is moreover the prevailing rock of the range. In the quarry on the side of the road to Ledbury, hornblende rock supports the stratified “granite;” and in Castle Morton quarry, on the eastern side of the range, large blocks of red felspar enclosing quartz and calcareous spar, are imbedded in hornblende rock. These masses sometimes appeared like short thick veins crossing each other in various directions, of which the terminations were mostly visible. It appears, therefore, impossible to consider this red felspar as a granite, and probable that the only reason why many, if not most of the projecting rocks of the range exhibit a considerable proportion of this “granite,” is, that the hornblende by which it

was heretofore flanked, and perhaps covered, has been decomposed and converted into the red earth, every where visible beneath the verdure, and often to a considerable depth; and it is to this decomposition that we are to attribute not only the generally smooth surface of these hills, but also the existence of numerous masses of greenstone, sienite, and red felspar, in the valleys, on the sides of the hills, and which still remain in great quantity imbedded in the reddish earth: all these portions of rocks are still angular, without exhibiting any appearance of having suffered by attrition.

The alleged origin of this loose reddish earth seemed the more plausible from finding that the hornblende rocks, which were in a state of decomposition, yielded an earth of the same colour and appearance beneath the hammer. Near the summer house on the top of the ridge, west of Little Malvern, it contains portions of a rock which have greatly the appearance of mica slate, and also masses of white quartz; while on the western side, near the foot, columnar masses of sandstone mostly quadrangular, and sometimes a foot in length, and containing internal ochreous spots, are found in loose earth beneath the verdure.

The foregoing facts, together with an examination of the projecting rocks of the range, of the varieties of which some account is annexed, appear sufficient to induce the conclusion, that all are to be considered as sienitic, or belonging to the trap formation, but of a peculiar character.

Annexed is a sketch of a mass of highly crystalline hornblende, about four feet long, and three feet in other directions, which I observed lying in the valley between North Hill and End Hill. The "veins" were of red felspar, enclosing hornblende.



This sketch will serve as a fair specimen of the general directions of the "granite veins" of this range.

In other masses, the veins were of epidote.

Rocks of the Malvern Hills, and their principal Localities.

Hornblende is the prevailing substance of the rock at Castle Morton quarry on the eastern side of Swinnit Hill. It is highly crystalline at that quarry, and sometimes contains roundish masses of calcareous spar, in other places specks of red felspar. It is sometimes traversed by red felspar in every possible direction, in veins which cannot be considered other than contemporaneous, from the 16th of an inch to a foot in thickness, and rarely of any considerable length, and often terminating abruptly. Thin veins of calcareous spar traverse the red felspar in various directions, striated contrary to the run of the vein; it often contains hornblende; rarely mica. It is also the prevailing rock of the quarry at

the northern part of the range. Hornblende rock passes into a

Substance of a dark green colour, imperfectly slaty texture, and earthy fracture, and of smooth surface, abounding at the Wych," and having the appearance of forming a bed there: it is interstratified with beds of red felspar, a little south of the four mile-stone between Great Malvern and Ledbury. That hornblende passes into this substance will become manifest by the use of the hammer at the Wych.

*Greenstone and sienitic rocks, both large and small grained, abound on the sides of End Hill and North Hill, and occur on their summits. Very fine grained greenstone occurs in a columnar form, breaking readily parallel to two of its planes, and sometimes in the form of an obtuse rhomboid, as near the summit of the Worcestershire Beacon, and on End Hill. A hard and somewhat schistose rock of hornblende and felspar in minute grains occurs *in situ* on the western side of the range, south of the Worcestershire Beacon: on the eastern side, a little south of Great Malvern, are rocks of crystalline hornblende, enclosing specks of red felspar and quartz, the mass being traversed by veins of epidote. Slaty hornblende enclosing specks and larger portions of felspar occurs at the Wych.*

Hornblende, reddish felspar, and quartz, "in small grains, constitute some of the rocks of End Hill and the summit of North Hill, and form a prevailing rock of these hills. It sometimes contains magnetic pyrites, veins of epidote, and sulphate of barytes."

Hornblende, felspar, quartz, and a little mica, "constitute the rocks on the west side of End Hill; and on the side of the road leading up to the Wych," hornblende prevails in the latter, and the rock is schistose.

Hornblende, with a few spangles of mica, and a little felspar, "on the ridge connecting North Hill and End Hill."

Hornblende and mica "are the constituents of rocks on the top of the hill between the Worcestershire Beacon and the Wych."

Hornblende and mica, "in a state of decomposition, mixed with red felspar; rocks of these constituents, and of a slaty structure, occur on the north-east side of the Worcestershire Beacon, and on the road leading from Great Malvern to St. Anne's Well."

Hornblende and epidote, "with specks of mica, and containing veins of epidote, constitute rocks on the north side of End Hill." Rocks of highly crystalline hornblende enclosing specks of red felspar and epidote (sometimes without the latter), are found in various places near the northern termination of the range.

Compact felspar, "of a pale flesh colour, is the prevailing

Fig. 1.

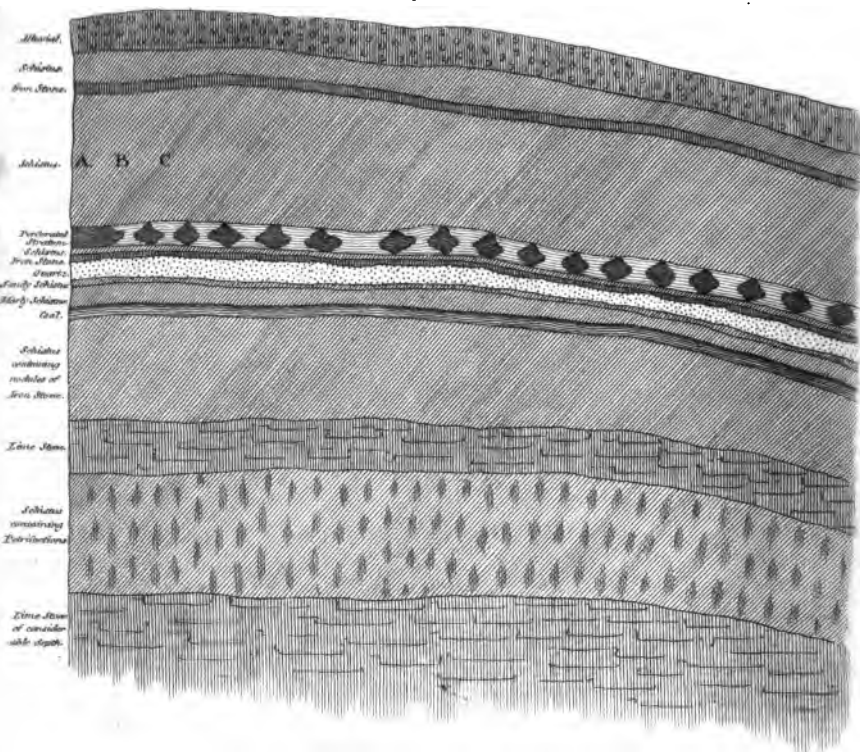
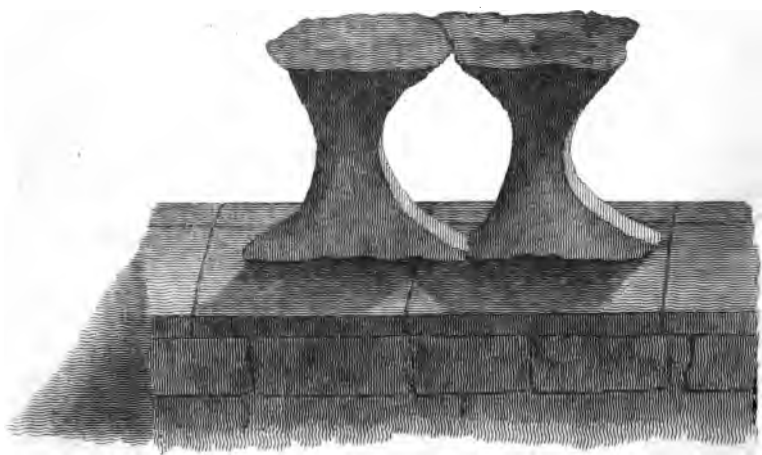


Fig. 2.



Remarkable Stratum of Limestone?

rock on the side of the road as it rises along the side of the valley above Little Malvern, and winds round the northern face of the Herefordshire Beacon."

Felspar and quartz, "with a little *minerals* and *epidote*, are described as principally composing the rocks of the western side of the range."

Compact felspar, hornblende, quartz, and straitite, "of an earthy texture, imbedding detached crystals of felspar, form a rock on the south side of Holly Bush Hill."

Opaque quartz and silvery mica, "in the form of a vein, occurs on the side of the road leading up to the Wych."

Felspar and mica, "united by a ferruginous clay, as far as the closeness of its texture would admit of decision, formed a massive rock exposed on the south end of the range, called Ragstone Hill by the quarriers. The rock is of an olive-green colour, and is occasionally traversed by veins of calcareous spar."

Conglomerate, "fine grained, of a dark-brown colour, and composed of felspar, steatite, and calcareous spar, united by a ferro-argillaceous base, and containing some minute specks, of a greenish yellow substance, in diverging fibres, which is probably actinolite. This rock, which occurs a short way to the south of the Herefordshire Beacon, attracts the magnet."

Conglomerate, of rounded masses and crystals of quartz and felspar, with some hornblende united by an argillaceous cement, in the new road lately made on the side of North Hill.

ARTICLE III.

An Account of a remarkable Stratum of Limestone, situated at Calder Side. (With a Plate.)

THE stratum of which, in the following pages I have endeavoured to give some account, is situated in the farm of Calder Side, in the parish of Kilbride, about 10 miles to the south of Glasgow; and occurs in the position noted in the following table:

	Feet. inches.	
Alluvial earth,		
Bituminous shale.		
Bestard ironstone, and reddish-black bituminous schistus	9	0
Perforated stratum, to be described	1	0
Very thin stratum of reddish-black schistus.		
Ironstone.	0	2½
Schistus abounding in quartz sand.	0	6

	Feet.	Inches.
Intercepted stratum of quartz.		
Marly schistus, intermediate in colour between greenish-grey and yellowish-grey, and containing the charred remains of vegetables.	0	10
Coal.	0	1 $\frac{1}{4}$
Bituminous shale.	0	2
Bituminous shale, differing from the preceding in being filled with nodules of ironstone ..	11	1
Limestone.		
Schistus containing petrified entrochi, but no ironstone nodules.	4 $\frac{1}{2}$	0
Lime of considerable thickness, and wrought for the purposes of sale.		

These strata are laid open to view on the side of a steep bank overhanging the stream of the Calder, on Mr. Young's property of Calder Side; and a short distance further up the stream than Calder Wood, the seat of Sir W. Maxwell.

The stratum of *perforated* limestone is the phenomenon which here attracts the notice of the naturalist. A representation of its appearance, as seen at Calder Side, is attempted in the sketch of the section of the strata (Pl. I.) mentioned in the table, and I shall request your indulgence in my endeavours now to describe it at somewhat greater length.

To account in a satisfactory manner for the formation of any stratum, or to explain the causes which have produced the most common geological appearances, are, perhaps, equally beyond the power of human talent, as to account for the formation of the perforated stratum of Calder Side. For these reasons I shall endeavour to confine myself to a mere description of the appearance of this stratum, and avoid any speculations as to its origin. It must be owned at the same time that while the common occurrence of many wonderful and inexplicable geological phenomena divest them in our eyes of the remarkable features which they in reality possess, that the imagination is almost irresistibly set at work, when so singular an appearance presents itself, as the one now under consideration.

This stratum probably consists of millions of blocks resembling those figured at fig. 2, for a great part of it is still covered by the superincumbent strata of schistus, &c. When observed in their natural position, these blocks are placed on end, their upper and lower ends forming the upper and under superficies of the stratum, which is here nearly in a horizontal position, and when first exposed by the removal of the superincumbent strata has the appearance of a pavement similar to some parts of the Giant's Causeway, from the ends of the blocks being so exactly fitted, and dovetailed into each other. This pavement is, however, hollow, and a labyrinth of concealed apartments exists in its interior, for the blocks which at their extremities are nicely fitted

into each other are worn away, as it were, at their centres into the form represented at fig. 2, and fig. 1, section of the perforated stratum.

The hollow parts of the stratum are filled with a fine earth, which has much resemblance to Armenian bole, and is coloured with iron, being stained and streaked, of various tints of red, orange, and yellow. This fine earth is constantly moist, and as soon as it is exposed to the air, it becomes covered with a luxuriant coating of vegetation, consisting of a minute species of conferva, whose thin roots, resembling the fibres of a spider's web, penetrate it in all directions. Such an appearance would have afforded matter of speculation to Duhamel and Henkel, and might have assisted these philosophers in their researches relative to what has been termed the equivocal generation of plants.* Have the diminutive seeds of these confervæ remained concealed for ages, locked up in the interior of a stratum of limestone, and buried beneath various strata of schistus and ironstone, and still retained the power of germinating as soon as exposed to the air of the atmosphere?

The upper and under superficies of the perforated stratum are thickly covered with petrifications consisting of a lesser variety of entrochi, and a quantity of shells of the genus ostrea, the substance of which does not appear to have been much changed by the petrifying process. At the time when these shells became imbedded in their present situation, the animal inhabiting them could not have been alive, as the valves are all of them found separate, and what is remarkable, those situated both on the upper and under superficies of the stratum have the interior superficies of the valve almost invariably turned towards the stratum. In the body of the blocks only a few entrochi, and none of the bivalve shells occur. A few of the shells and some of the entrochi coat the surface of the interstitial vacuities. What a strange variety of causes must here have been called into action to produce the effects to be observed here: on the upper and under superficies of the stratum we have petrifications resembling each other in their nature. In what may be termed the body of the stratum, we entirely want the more recent and perfect of these petrifications; viz. the bivalve shells, and have only a few of the entrochi.

The same law which seems first to have acted in the formation of the perforated stratum seems also to have exerted its influence, although only to a very limited extent, upon the superincumbent strata of schistus and ironstone; for these strata seem to show a tendency to separate into something of a crystalline structure, as it were approaching to very imperfect basaltic columns in the direction of the lines A B C, fig. 1.

If we could suppose that previous to the blocks composing

* See Henkel's *Flora Saturniana*, chap. ii. p. 35 and 36.

the perforated stratum having assumed their present form, they had been detached into distinct crystals by some process similar to what basaltic columns owe their origin to, part of the mystery might be considered as unriddled; for no accidental rocks could have separated the stratum into these distinct blocks; but still the blocks becoming each of them thinner towards their centre is to be explained. Could a stream of water percolating through the interstices have reduced them to this shape? I rather fear that the laws of hydraulics forbid such a supposition. The middle part only of each block is worn away; both ends remain entire. Must we not, therefore, conclude that some extraordinary operation depending on chemical laws has, at once formed each separate block into its present form? but here I must call to recollection the inadequacy of such speculations to explain the subject of the present paper, and confine myself to a relation of facts.

Many years ago I had an opportunity of viewing from a short distance an appearance which, if my memory does not fail me, bore a considerable resemblance to the perforated stratum of Calder Side. It was what seemed to be a range of holes resembling in form the holes made by the Sand Martin (*Hirundo Riparia*) in banks of sand, in which these birds construct their nests. The holes which I allude to were situated in the front of a steep rock overhanging the river Sotha, near the celebrated falls of Trollhatta. It was in passing down the river in the dusk of the evening that I saw them, so that I could not observe whether the rock was stratified or not; and indeed I should probably not at all have remarked them had not the boatman who conveyed me down the river pointed them out to me, remarking at the same time that they were the residence of spirits; and that during the fine nights in autumn, a bright light was seen to issue from them. Most probably our ancestors would at once have referred the perforated stratum of Calder Side to the times when various places in Scotland were peopled by the fairies of popular superstition, and dated its origin at the hands of supernatural beings, and explained its uses as forming the abode of the spirits of the wood, or of the flood.

Fig. 1, is a section of the strata at Calder Side.

Fig. 2, two of the blocks composing the perforated stratum of limestone, taken from their place, and seen in perspective as placed upon a wall near the spot. These blocks consist of what may be termed an argillaceous limestone of a bluish grey colour, and of a very slaty texture. It appears to contain much bitumen.

ARTICLE IV.

On the Action of Chlorides and Water.

By Richard Phillips, FRSE. FLS. &c.

DIFFERENT opinions appear to be entertained by chemists of the greatest eminence and experience as to the changes effected on those chlorides that are soluble in water by solution in it. On this account I propose to state such opinions as I have found detailed on the subject, and I shall endeavour to collect the evidence which has been adduced in support of the different views entertained.

The question to be solved may be thus stated: When a chloride is dissolved in water, does it remain a chloride, or is it by decomposing water converted into a muriate? It is further to be considered, whether the same explanation will apply to all the aqueous solutions of chlorides.

With respect to the non-metallic chlorides, it is to be observed that one of them, viz. chloride of azote, is insoluble in water, and consequently no change is effected in the properties of either compound. The chlorides of phosphorus and of sulphur on the other hand act with great energy on water, and offer incontrovertible evidence that water in these cases suffers decomposition, for the products are such as do not combine with each other, but exist in a state of mixture in the water, each possessing its peculiar properties. Thus when chloride of phosphorus and water undergo mutual action, the oxygen of the water forms phosphorous acid with the phosphorus, and the hydrogen unites with the chlorine to form muriatic acid. The perchloride of phosphorus effecting similar decomposition yields phosphoric and muriatic acids; and when chloride of sulphur and water undergo mutual decomposition, there are produced sulphurous, sulphuric, and muriatic acids.

Sir H. Davy (Phil. Trans. 1810) observes, that "when water is added in certain quantities to Libavius's liquor, a solid crystalline mass is obtained, from which oxide of tin and muriate of ammonia can be obtained by ammonia. In this case, the oxygen may be conceived to be supplied to the tin, hydrogen to the oxymuriatic acid." In the Phil. Trans. for 1810, he states more distinctly that Libavius's liquor is "converted into a muriate by water."

In his Elements of Chemical Philosophy, Sir H. Davy has been, in many instances, quite explicit on this point; and his opinions are favourable to the idea that chlorides become muriates by being dissolved in water. Thus he states that the perchloride of iron "acts with violence upon water, and forms a solution of red muriate of iron;" and he observes that the protomuriate "forms a solution of green muriate of iron by its action upon water."

In Dr. Thomson's System of Chemistry, I do not meet with any detailed opinion on the subject. He states, however, that "chloride of antimony is decomposed when mixed with water, white oxide of antimony, and muriatic acid, being formed."—(Vol. i. p. 584.) In treating of the muriates generally, he refers them to the chlorides, and of muriate of barytes he says, "this salt likewise will be found under the name of chloride of barium;" and he states that the "crystals of chloride gradually deposit." From this it would appear that the crystalline salt usually termed muriate of barytes is considered by Dr. Thomson to be a chloride; and in the *Annals* for November last he considers chloride of barium as converted into muriate of barytes by solution in water.

Mr. Brande in his Manual (p. 174), describing the properties of metallic chlorides, observes, that "some are soluble, others insoluble, in water. Several of them decompose water, giving rise to the formation of muriatic acid and an oxide; or in some cases to a muriate." Mr. Brande has not, I think, pointed out instances of the production of these different effects, but from his stating that chloride of potassium dissolves without decomposition in water, it would appear that he considers it to remain a chloride in solution. Mr. Brande, however, states distinctly that "when chloride of manganese is dissolved in water, it produces muriate of manganese." The chloride and perchloride of iron produce also he admits muriate and permuriate of iron when acted upon by water, and he allows similar decomposition of water, and the consequent formation of oxide and muriatic acid, to the chlorides of zinc, tin, copper, and antimony.

M. Thenard in the first edition of his *Traité de Chimie* (1816) states, without any reserve, that "all chlorurets, when dissolving in water, decompose it, and become hydrochlorates: they thus effect the decomposition of water in the same way as the iodurets, and the two principles of the water, the oxygen and the hydrogen, unite; the first with the metal, and the second with the chlorine." In the second edition of this work (1817), M. Thenard maintains the same opinion: he states, however, several of the difficulties which attend both opinions.

M. Gay-Lussac, on the other hand, in his memoir on iodine (*Annals*, vol. v. p. 125), observes, "We ought then to admit it as a certain fact, that the muriates are all changed into chlorurets, when melted, or even when dried, and some of them even by being crystallized. We may suppose, as we have done for the iodurets, that the chlorurets dissolve in water without undergoing decomposition, and that when we unite hydrochloric acid with an oxide, the hydrogen of the acid, and the oxygen of the oxide form water. Whether this be the case or not, nothing but chlorurets exist at a red heat." In subsequent parts of the same memoir, M. Gay-Lussac says, "I believe that according to the nature of the substance with which the chlorine is combined, the chlorurets may dissolve in water without undergoing

decomposition, or being changed into hydrochlorates during that solution." He further states, "I admit as a principle that we ought to have a chloruret or a hydrochlorate in solution, according to the forces which act in order to decompose water are greater or less than those which keep its elements united."

In attempting to elucidate this as well as every other subject, it will be better to begin with those cases which are too obvious to admit of question, and proceed to those which are more obscure. There are some cases of the action of water upon chlorides which prove, I think, incontestably the decomposition of the water, and the union of its oxygen with the metal, and its hydrogen with the chlorine—I mean the effects observed when the chloride of bismuth and of antimony are acted upon. It is quite evident that oxides of these metals are precipitated, and that muriatic acid remains in solution. It will be unquestionably admitted, whether we dissolve a metallic oxide in muriatic acid, or take the dry compound which remains after evaporating such solution, and exposing it to a red heat, and dissolve it in water, that the solutions obtained are in all respects similar. Thus when we dissolve peroxide of iron in muriatic acid, we obtain a red coloured solution; and a similar effect is produced, if we dissolve the perchloride of iron in water. Both solutions it will be admitted contain either chlorides or muriates.

When iron is put into dilute muriatic acid, it is well known that hydrogen gas is evolved, and the iron dissolved. Now if we conceive this solution to contain chloride of iron, we must suppose that the hydrogen evolved is derived from the decomposition of the muriatic acid, and not of the water; and as we obtain a similar solution by dissolving protoxide of iron in muriatic acid, we must suppose that the oxygen of the oxide unites with the hydrogen of the muriatic acid, that water is formed, and chloride of iron remains in solution. I say we must admit the evolution of hydrogen from the decomposition of the muriatic acid in the first case, or we must make the improbable supposition that iron, while dissolving, decomposes water to receive oxygen from hydrogen, and that immediately afterwards it yields the oxygen to the hydrogen of the muriatic acid, and thus produces chloride of iron.

From these considerations, I think it will involve fewer difficulties to consider this solution as containing a muriate rather than a chloride. We have in this case only to admit, as is indeed generally allowed, that the hydrogen is evolved from the decomposition of the water, and not of the acid; and when the oxide of iron is dissolved in liquid muriatic acid, we must consider that the oxygen of the oxide and the hydrogen of the acid do not form water, but remain combined, the first with the iron, and the hydrogen with the chlorine.

There are other considerations which tend to strengthen the opinion that the chlorides of iron become muriates by decomposing and dissolving in water. If to these solutions we add an alkali as potash or soda, oxide of iron is precipitated. Now we must either admit that oxygen is transferred from the alkaline oxide to the iron, which is a possible case, that the iron previously existed in the state of oxide, or that by some operation similar to that which has been called predisposing affinity, the decomposition of water is effected at the moment of the presentation of potash to chloride of iron.

As ammonia contains no oxygen, and as it decomposes solutions of iron as readily as potash, the cases are reduced to two, viz. either that the iron exists in the state of oxide, or that it becomes so by the intermediate action of the ammonia. It appears to me that the first is the most probable case, although, as I shall presently notice, the latter is a possible one. Among the reasons which may be advanced for supposing the solution to contain a muriate are the facts already adverted to of the decomposition of chloride of antimony by water; another reason is that there are some acids which form insoluble compounds with the oxide of iron, such as the phosphoric, for example. If then we add a solution of phosphate of soda to a solution of muriate or chloride of iron, phosphate of iron is precipitated, which consists of the acid united to oxide of iron, and we are, therefore, reduced either to admit that the iron exists in the state of oxide, or that chloride of iron decomposes water by the addition of a solution of phosphate of soda, for which there appears to be no sufficient cause, nor do I recollect the occurrence of any such case.

The action of water upon the chlorides of potassium, sodium, barium, &c. does not appear to have been more decided upon by chemists than other chlorides. Dr. Thomson, as I have already noticed, considers that chloride of barium by solution in water becomes a muriate; whereas M. Gay-Lussac, in a note to a memoir I have already alluded to, says, "On mixing solutions of chloruret of calcium and sulphate of ammonia nearly in equal volumes, the temperature scarcely rose one degree of Fahrenheit, though such a quantity of sulphate of lime was formed that the whole mixture became solid. The solution of chloruret of barium treated in the same way produced an elevation of about 6.3°. From these it would seem that in the solution of chloruret of calcium, the metal is in the state of an oxide, while in that of chloruret of barium the metal is still in the metallic state." With respect to the chloride of barium, I am certainly much more inclined to adopt the opinion of Dr. Thomson than of M. Gay-Lussac; for it appears to me extremely difficult to discover by what kind of action water is decomposed when sulphate of soda, for example, is added in solution of chloride of barium, and

indeed this very decomposition on this principle must, I think, prove fatal to what seems to be M. Gay-Lussac's opinion that chloride of sodium is not decomposed by water. If this latter supposition be true, then, when sulphate of soda and muriate of barytes suffer mutual decomposition, the muriate of soda which is formed must exist as such only for a moment, and water must be recomposed to form the chloride he supposes to exist.

That the different temperatures which are occasioned by dissolving certain chlorides in water cannot, I think, be deemed a criterion for determining the question may be inferred from a very ingenious paper, contained in vol. xii. p. 42, of the *Annales de Chimie*. In this memoir, upon the analysis of mixtures of the chlorides of potassium and sodium, it is stated that under similar circumstances a given weight of chloride of potassium sinks Fahrenheit's thermometer 20.52° , and the same quantity of chloride of sodium depresses it under the same circumstances only 3.42° . Now it can, I think, hardly be supposed that bodies so similar in their affinity for oxygen, as potassium and sodium, should differ in their action on water when combined with chlorine. We may consider these chlorides and that of barium as converted into muriates by solution.

With respect to the chloride of potassium and sodium, there are other reasons for believing them to be converted into muriates by solution in water. In the first place, these metals have strong affinity for the oxygen of water, and so also has the chlorine for its hydrogen, becoming muriatic acid when its aqueous solution is exposed to the solar light. As, however, chlorine and these metals have also great affinity for each other, it is certainly possible that this mutual affinity may diminish or destroy their separate attraction for hydrogen and oxygen. It must be granted, as before noticed, that whether we dissolve dry chlorides in water, or saturate liquid muriatic acid with metallic oxides, that similar solutions are produced.

If to the solutions of chloride of sodium or potassium we add sulphuric acid, their respective sulphates are formed, and a similar effect, *mutatis mutandis*, is produced when tartaric acid is used with the solution of potash.

Now in these operations, one of two cases must occur. Supposing we have dissolved potash in muriatic acid, and the result to be solution of chloride of potassium, water must have been formed, we then add tartaric acid, and this possesses the power of so acting upon the elements of the solution that water is again decomposed, and muriatic acid and potash again result, as is evident by examining the solution and the crystalline deposit of the bitartrate of potash.

The other case is simply this; viz. that the chloride of potassium immediately decomposes water, and then there is no necessity for attributing this power to the intervention of the sulphuric or tartaric acid.

There is one case which I have already hinted at which appears to me difficult of explanation upon any supposition. When chloride of mercury is put into water, as already noticed, neither solution nor action takes place: if we add potash to the mixture, protoxide of mercury is immediately precipitated. Now in this case it would certainly appear to be most probable that the oxygen is supplied by the potash, the chloride of potassium formed remaining as such in solution. If, however, we substitute ammonia for potash, still the decomposition is effected, muriate of ammonia is formed, and protoxide of mercury precipitated. In this case it would seem that water is decomposed by the intervention of ammonia; for not containing any oxygen, it cannot yield it, as the potash may be supposed to do. It is difficult, I think, to explain this action, and on this account I am far from denying the possibility of tartaric acid to effect that decomposition of water which may be supposed to occur when it acts upon chlorine, potassium, and water. Certainly what ammonia appears to do in the case of chloride of mercury, tartaric acid may effect in that of chloride of potassium.

In vol. vi. p. 185, of the *Annals*, M. Gay-Lussac says, "When a solution of chloruret of calcium (which he supposes to exist in solution as such) is mixed with subcarbonate of ammonia, the chlorine must pass to the state of hydrochloric acid, in order to combine with the ammonia;" and further on, he observes, "It is the difference of solubility of subcarbonate of lime and hydrochlorate of ammonia, which occasions the double exchange of the bases and acids; and consequently it is on account of that difference of solubility that water is decomposed."

Now with much deference I think this reasoning can scarcely be admitted. Surely the insolubility of carbonate of lime cannot act until the carbonate exists, and the decomposition, for which this insolubility is adduced to account, must occur before it. If, however, the kind of reasoning employed by M. Gay-Lussac is admitted, then we may certainly account for the decomposition of chloride of mercury, by supposing that it results from the affinity of uncreated muriatic acid for ammonia.

After having considered the subject (I confess with much less attention than the intricacy of it requires), I incline to the opinion that all soluble chlorides are converted into muriates by solution in water. I think it will appear, from what I have stated, that fewer decompositions must be supposed to occur on this supposition than the other; and it is to be recollected, in support of this opinion, that no objection, as far as I know, has been made against the idea of the decomposition of water as it respects sulphuret of potash, or rather potassium.

Admitting (what, however, I am by no means disposed to assert) that the opinion which I have adopted to is most probable, the question still remains to be decided, under what point of view shall we regard those chlorides, or muriates, which

contain water of crystallization. Are they in their crystalline state chlorides or muriates? I certainly in this case also incline to the opinion that they are to be regarded as muriates. This idea is much strengthened by the following passage from Dr. Thomson's paper, on the "True Weights of the Atoms of Barytes, Potash, &c." (*Annals*, vol. xvi. p. 331), "When crystallized muriate of barytes is exposed to a red heat in a platinum crucible, it loses all its water of crystallization; while at the same time the hydrogen of the muriatic acid unites with the oxygen of the barytes, and flies off in the state of water."

ARTICLE V.

Extract from "An Account of Two late Attempts to ascend Mont-Blanc, by Dr. Hamel, Counsellor of State to his Majesty the Emperor of all the Russias."

MOST of our readers are probably aware that during the last summer an attempt was made by Dr. Hamel, in company with several other persons, to reach the summit of Mont Blanc: it is equally well known that during the journey, the whole party was in the most imminent danger, and that some of the guides actually lost their lives. It is presumed that the particulars of this fatal expedition, taken from the *Bibliothèque Universelle* published in August last, will be acceptable to the readers of the *Annals*.

The first of the two attempts of which an account is given was undertaken on August 3, the anniversary of the ascent of M. de Saussure. Dr. Hamel, in passing by the baths of St. Gervais, heard that two persons of the country had reached the summit of Mont Blanc, and descended the same day at Prarion, whence they had set out. This report made Dr. Hamel desirous of attempting this new route, which, according to the account given of it, was less difficult, and dangerous, and much shorter, than that by Chamouny, which, since Saussure's time, had always been followed.

It appears that the same persons who had already ascended by this route proposed again to undertake it, in order to remove some doubts which had been entertained at Chamouny with respect to the practicability of the journey. With these persons and the Curates of St. Gervais, and St. Nicolas de Verosse, the attempt was made by Dr. Hamel.

In order to insure success, it was proposed to effect the ascent in two days, passing the night near la Pierre Ronde, the name given to some rocks situated beneath L'Aiguille du Gouté, and at half-past seven, the travellers halted to pass the night, sheltered

crossed by the rocks, and not far from a torrent coming from the glacier de Bionnassay.

The night was beautiful, and at half past two in the morning, the journey was resumed by moon-light, and at 22 minutes after five o'clock, the travellers reached the base of the Aiguille du Gouté; and after about three hours of very difficult ascent among loose stones, the summit of the Aiguille was attained at 60 minutes after eight; the height of which is upwards of 12,000 feet. After taking rest, and adopting precautions against the cold, and the rays of the sun reflected by the snow, they set off at a quarter past nine towards the Dôme du Gouté, and arrived at the summit at half past 11.

The height of this is 1330 [13,300] feet, and Dr. Hamel could advance but few steps without waiting to take breath, on account of the rarity of the air; and finding, from the state of exhaustion he was in, that he must have staid at least half an hour before he could resume his journey towards the summit of Mont Blanc, and calculating that it would be impossible to come back to the Aiguille du Gouté before night, he resolved to return by the route by which he had ascended, without attaining his object. The descent is represented as more difficult and dangerous even than the ascent, but it was safely performed, and the party arrived at about nine at night at an inn called Pavillon de Bellevue, situated on the Montagne de la Chaletta between Mont Lacha and Mont Prarion.

Having given a sketch of Dr. Hamel's account of his first attempt to ascend Mont Blanc, I shall continue nearly in his own words the account of his second journey, and which was attended with great danger to the whole party, and proved fatal to some of the guides.

"In looking over M. de Saussure's work, I found that he had tried this same route; but the dangers which he encountered in the ridges of l'Aiguille du Gouté prevented him from proceeding further: he did not even arrive at its summit.

"This induced me to believe that the route by Chamouny, by which he afterwards ascended, must be at least as convenient; and I wished to meet with an opportunity of trying it, so that I might decide which of the two routes was preferable.

"Soon afterwards, I learned that some persons at Geneva were also desirous of ascending Mont Blanc: one of them was M. Selligie, a mineralogist and mechanical artist. He informed me that he had invented a barometer upon an entirely new principle, which he wished to try on these mountains.

"As the claim of Mont Blanc to be considered the highest mountain of Europe has been lately disputed, and as no travellers who have ascended it since M. de Saussure have taken the trouble to measure it again, I wished to determine its height, with the assistance of several barometers. Prof. de Saussure had the goodness to lend me an excellent walking-stick baro-

meter, made at Turin. The reservoir for the mercury is a glass cylinder, and the level is regulated by a screw and piston. M. Selligie constructed a syphon barometer; and in case these two barometers should be deranged in ascending, I filled with mercury, two glass tubes 18 or 20 inches in length, and bent at one end like a syphon. The mercury having been boiled, I closed the opening, so that no variation in the volume of the mercury might cause air to enter the longer leg of the syphon. On arriving at any height, I had only to remove the cork, and allow part of the mercury to come out, and then to measure the height of the column standing in the tube. I had thus four barometrical instruments to measure the height of the summit.

"In my first ascent I was surprised at the action which the sun's rays had upon the skin; and I intended to make some experiments on the power of these rays concentrated by lenses, Col. Beaufoy had previously paid some attention to this subject, and I think, as he does, that these experiments may become interesting in the theory of light and heat.

"I purposed also to make observations upon myself and my companions as to the effects of rarefied air upon animal organization; and after what I had already observed during my first ascent, I flattered myself that I should obtain results which would be useful in physiology.

"I procured a bottle of lime-water to determine the presence, and by approximation, the quantity of carbonic acid in these elevated regions, and to discover whether air which had been respired contained the same quantity of carbonic acid as it does in those regions in which at each inspiration one-third more oxygen enters in the same volume of atmospheric air. I intended also, when high up, to bleed some animal, in order to observe by the colour of the blood whether it was or was not sufficiently decarbonized.

"I filled four bottles with spirit of wine, which, when poured upon a sponge, was intended to be used for combustion; and I proposed to bring back some of the air of the summit in these bottles for analysis.

"A Papin's digester, of very simple construction, was intended to prove the possibility of cooking meat at great heights. The monks of the Grand St. Bernard complain that they are unable to dress their food sufficiently. The reason of it is, that water in large open vessels, being less compressed by the atmosphere at great heights than in plains, boils at a lower temperature. A separate apparatus was prepared to measure the exact temperature at which water boils at different heights.

"A small table, with a camera lucida, was furnished by M. Selligie, to sketch a panorama from the summit of Mont Blanc.

"Prof. Pictet supplied me with the instruments requisite to observe and measure the temperature, the electricity, and the moisture of the atmosphere, &c. &c.

"Mr. Joseph Dornford, and Mr. Gilbert Henderson, two English gentlemen, and both of the University of Oxford, were anxious to join us; the former of them had, when in England, formed the project of ascending Mont Blanc. We set out on Aug. 16, at three o'clock in the afternoon from Geneva for Chamouny. We arrived the next day at le Prieuré, reaching the excellent hotel de l'Union, kept by M. Charlet, at two o'clock.

"We applied to Joseph-Marie Coutet et Mathieu, son of Pierre Balmat, whom M. Pictet had recommended to us as guides who were equally robust and trusty. They advised us to take 12 guides, or three for each traveller. We referred the choice to them; and our obliging hostess undertook to prepare all that was necessary for the journey. The next morning at a quarter past five o'clock, we set out in the finest possible weather. We went at first in the direction of the Glacier des Bossons, but before we reached it, we turned to the left, and began the ascent in a forest. At seven o'clock, we had got above the forest, and reached the chalet, inhabited by Pierre François Favret, formerly one of Saussure's guides, who had ascended, and his son was with us.

"Here one of our guides, Julien Devouassou, son-in-law of D. Paccard, was nearly poisoned. He supposed that he had bought some syrup of vinegar at Chamouny; and arriving at a stream, he tried the syrup before he mixed it with water: he swallowed a little of it. It was concentrated sulphuric acid, which burned his stomach and mouth in a terrible manner. His sufferings were great, and he vomited much. Fortunately this accident happened near a chalet, where I found some wood ashes, and these I made him swallow mixed with water; the alkali neutralized the acid instantaneously, and the guide having recovered, continued the journey with us.

"From the chalet, the ascent is continued zig-zag in the direction of the Aiguille du Midi; at half-past eight, we rested at la Pierre-pointue, where the mountain projects between the Glacier des Bossons and the Glacier des Pelerins, but nearest the former. From hence the summit of Mont Blanc is for the first time visible, and le Prieuré is still in sight. From thence we turned a little to the right, and at nine o'clock we crossed the torrent called l'eau noir, but which, instead of water, was filled with enormous blocks of granite, rolled down from above: the Aiguille de la Tour is on the left. A quarter of an hour afterwards, we passed to Nant Blanc; and at a quarter before 10 o'clock we halted to breakfast round a great stone between the Glacier des Bossons and Mount Basselache. The guides call it "la pierre de l'échelle," because they usually leave the ladder there which is used to cross the Glacier. At 10 minutes past 11 o'clock we recommenced our journey, and in five minutes we reached the Glacier des Bossons, which must be crossed obliquely in the direction of the Grand Mulet. At a

hundred paces from the edge of the Glacier wonders begin which no pen can describe. At every moment we stopped to make each other observe some striking configuration of the ice; we heard nothing but 'Look to the right! Look to the left!' Sometimes it was a bottomless precipice; at others a tower of ice more than 100 feet in height. The ladder was soon required. How were we to cross a crevice of 20 feet wide, apparently bottomless, longitudinally divided into two by a thin wall of ice, scarcely a foot thick, and 10 feet lower than the sides of the crevice which it divides? Arrived at the brink the ladder is lowered and supported upon the thin wall of ice in the middle of the crevice. One of the guides descends; the first traveller follows, and keeps himself upright near the ladder on the wall of ice, one foot broad, or supported by his stick, he stands immovable, and endeavours to shun the sight of the two blue gulphs ready to swallow him on the least loss of equilibrium. The guide then rests the top of the ladder against the opposite edge of the crevice, and the traveller having mounted it, it is again returned to the first brink in order to let the second traveller descend, and so on with the remainder. When the ladder is some inches longer than the crevice is wide, it is placed across it like a bridge; and every one goes over on his hands and knees. Sometimes there are crevices which are covered by bridges of snow which are often narrow. Sometimes those who walk last find these bridges penetrated by a foot which has preceded them, and it is then necessary to turn the foot a little aside.

"In spite of all the difficulties and dangers, we crossed the Glacier without the least accident. At a quarter past one we were above the junction of the Glacier des Bossons with the Glacier de Tacconay, and between these two occurs the Montagne de la Côte; and after having ascended a ridge of snow, inclined at an angle of 56° , at precisely three o'clock, we set foot on the base of the Grand Mulet on the western side. These rocks, which project from the middle of the ice, are not more firm than those of the Aiguille du Gouté; and we ascended them so slowly that it was half-past four when we arrived at the highest part of the Grand Mulet. A black cloud which formed in the west decided us to stay here for the night. The summit of this rock having the form of the letter L; that is to say, of a right angle, our ladder and some sticks covered with cloth were so arranged as to form the hypotenuse. A little straw scattered upon the horizontal part of the rock was the mattress upon which we lay down side by side. We were hardly covered when it began to rain; and soon after the thunder was heard majestically around us. I had scarcely attempted to put the point of the electrometer out of our tent, when its two balls began to move with so much violence that I was alarmed. The whole of the night was stormy. The next morning the rain

ceased; and the air was so pure that we saw the lake of Geneva, and some more distant objects, very distinctly.

"We hoped that towards noon the weather would clear up, but it remained uncertain, and we resolved then to bivouac again the following night in our tent à la Cossaque. Coutet sent two of our men to Le Prieuré to procure a supply of provisions for the guides. At intervals, I occupied myself with reboiling the mercury of one of my barometer tubes, which had been injured on the journey. We tried the temperature of boiling water, and found it to be 162° of Fahrenheit. I made arrangements to let off some fire-works on returning from the summit, for I was curious to try whether the fire-works would rise well in this rare air. I had balls and shining stars prepared with arsenic. I had also Bengal fire prepared with antimony; and, besides, a mixture of nitre, sulphur, and orpiment, in order to try to illuminate all the summit of Mont Blanc, and the spires of the surrounding mountains.

"At five o'clock some hail fell, and until midnight the weather was cloudy; but on the 20th, at one in the morning, the heavens appeared studded with stars, although the valley was still hidden by fog. Coutet went out to observe the weather, and informed us that it promised well, but that it would be prudent still to wait a little before we decided upon ascending. At about five o'clock, the summit was lighted by the sun; the air perfectly serene and calm. Our guides informed us that we might begin our journey. M. Selligie, who had been for some time unwell, and who feared the return of the rain before the close of day, preferred staying at the Grand Mulet. Two of our guides who had never ascended Mont Blanc, and whom Coutet wished to remain with M. Selligie, refused to do so. The weather was favourable, and every one was desirous of ascending. At length two other guides consented to stay behind. Messrs. Dornford, Henderson, and I, with eight guides, set out from the Grand Mulet at 20 minutes past five; the thermometer was at 34° of Fahrenheit. Having entered upon the snows which were here rather deep, we went immediately towards the Aiguille du Gouté, afterwards we ascended towards the summit, and almost always in a zig-zag direction, in order to avoid the crevices and steep acclivities; and at a quarter before seven, the summit reappeared on this side. The weather was beautiful; and, very far beneath us, we saw white clouds, like a calm sea, penetrated here and there by the summits of the highest mountains, the names of which were mentioned by Coutet, les Fours, l'Aiguille de Varens, le Buet, le Dent du Midi, le Dent de Morcle, &c. At about seven o'clock these clouds began to disperse, and we perceived le Prieuré. As we continued to ascend, we found the snow harder, and not so deep. No snow seemed to have fallen in these higher parts for some time.

At 20 minutes past seven we reached the first of three pit-

forms of snow, which occur in the space between the Dôme du Gouté and Mont Maudit, the eastern shoulder of Mont Blanc. After having crossed this first platform, at a quarter before eight, we ascended an acclivity at an angle of 25° or 30° . This leads to the second, which we began to cross at 10 minutes past eight, having then on our right hand the great *seracs** of ice which are visible even at Chamouny. The sky, when observed near these white masses of ice, appears of an extremely deep blue colour; indeed almost black. After having ascended another steep acclivity, we reached, at half-past eight, the last of the great platforms, bounded on the right by the highest part of the Dôme; on the left, by the last rocks on this side, and on the south side by a steep acclivity, at about the height of which, and a little further, appears the summit of Mont Blanc. Here our guides congratulated us, telling us that all difficulties were actually overcome; there were no more crevices, no more dangers. Never, said they, did an ascent succeed better; no one ever ascended more rapidly, and with less difficulty. In fact, the snow had precisely the degree of hardness which is desirable for walking upon with ease: the feet did not sink too much, and the snow was not too hard. We had nevertheless for some time experienced the effects of the rarity of the air: my pulse beat 128 in a minute, and I was continually thirsty. Our guides advised us to breakfast here, for, said they, higher up you will have no appetite. A cloth was spread upon the snow at the entrance of the great platform, which served both for chairs and table. Every one eat his half chicken with appetite. I arranged several things for the observations and experiments which I proposed to make on the summit. I wrote two notes to announce our arrival at the summit, leaving a blank merely to insert the hour. I intended to fasten them to a pigeon which I had with me, and which I intended to liberate on the summit, in order to observe how he would fly in this rare atmosphere; and afterwards to know if he would find his way to Sallenche, where the female was. We kept a bottle of our best wine in order to drink to the memory of de Saussure on the summit.

“At precisely nine o'clock, we set off to ascend the summit, which we saw before us. “Would you accept a thousand pounds to descend instead of ascending?” said one of my companions to his fellow countrymen. “I would not return for any money,” was the reply. We were all full of hope and joy at seeing ourselves so near the end of our journey. The beautiful weather, the calm which reigned around us, the celestial air which we had breathed during our repast, made impressions upon our minds which are not experienced in lower regions.

* *Seracs* are those parallelepipeds, cubes, and other rather regular forms of ice and snow which are found at great heights. The name is derived from a kind of white cheese, made in the mountains, and to which similar forms are given.

I saw myself already on the summit. I took a specimen of the highest rock in Europe to place it in the Imperial Mineralogical Cabinet at St. Petersburg. I intended some for the museum at Geneva, and other collections.

"We crossed the great platform of snow, at the entrance of which we had breakfasted. While crossing, I had occasion to remain for some time rather behind, and it was near the angle on the right that I rejoined the company. We ascended about half the height of the great acclivity of snow, which, extending the whole length of the platform, rises towards the summit of Mont Blanc. As, however, between this acclivity and the summit there are ridges of ice which are almost vertical, it is necessary to cross the acclivity horizontally, by keeping to the left, in order to reach the last great rocks at the height of 14,700 feet from which Italy is visible. From these rocks, by turning to the right, the summit is ascended, at the height of 960 feet. We walked one after the other, for it is preferable to tread in the footsteps of the first guide, who, on account of the fatigue which he suffers, is changed from time to time.

"We advanced thus in a nearly horizontal line, crossing the acclivity about half way up; that is to say, at almost equal distances between the ridges on our right, and the platform of snow upon our left. No one spoke, for at this height even talking is fatiguing, and the air conveys sound but faintly. I was still the last, and after taking a dozen steps, supported by my stick, I stopped to make 15 inspirations. I found that in this manner I could advance without exhaustion. Prepared with green spectacles, and with crape before the face, my eyes were fixed upon my steps, which I counted. Suddenly I felt the snow give way under my feet. Thinking that I merely slipped, I thrust my stick to the left, but in vain. The snow, which accumulated on my right, overturned and covered me; and I felt myself forced downwards with irresistible power. I thought at first that I was the only one who suffered this accident, but feeling the snow accumulate upon me so as almost to prevent my breathing, I imagined that a great avalanche was descending from Mont Blanc, and forcing the snow before it. Every moment I expected to be crushed by this mass: while descending I turned over repeatedly, and I strove with my strength to divide the snow with my arms, in which I was buried and struggling. I succeeded at last in getting my head out, and I saw that a great part of the acclivity was moving; but as I found myself near the edge of this slippery part, I made every effort to get upon the hard snow, where I might at last find footing. It was not until then that I knew the extent of the danger; for I saw myself near a crevice which terminated the acclivity, and separated it from the platform. At the same moment I saw Mr. Henderson's head still nearer this abyss. I discovered still further Mr. Dornford and three guides, but the five others did not appear."

hoped still to see them come out of the snow which had stopped them, but Mathieu Balmat cried out that there were still persons in the crevice. I will not attempt to paint what then passed in my mind. I saw Mr. Dornford throw himself upon the snow in despair, and Mr. Henderson was in a condition which made me fear for the consequences. But our consolation may be judged of when, some minutes afterwards, we saw one of the guides come out of the crevice; our hurrahs redoubled at the appearance of the second; and we yet hoped that the three others would also reappear, but, alas! they were seen no more.

The guides, fearing a second slipping of the snow, directed us to remove to a distance, but that was impossible. Mr. Dornford declared that he was ready to sacrifice his life to go and search for the unfortunate guides: I offered him my hand, and partly sunk in the snow, still moveable, we advanced in spite of the guides, towards the crevice of unknown depth filled with snow, and to the place in which they must have fallen. There we descended into this gulph, and I sounded the snow every where with a stick without feeling any resistance. Supposing it possible that the men might have fallen into some cavity, or upon some projection in the crevice; and as the air, on account of its rarity, does not convey sound well, I thrust the longest stick quite to the end in the snow; and lying down upon it, I applied my teeth to the stick, and calling the men by their names, I listened with great attention to hear any noise; but all was in vain. The guides came upon us, and forced us, so to express it, to come out of the crevice. They declared our search useless; they even refused the money that we offered them if they would wait; they laid hold of MM. Dornford and Henderson; and while I was still sounding the snow (which had passed the crevice for a great space), they proceeded immediately with them to some distance; so that I was under the necessity of descending with only Coutet, who had not even a stick; but absorbed in the horror of the event, I was become insensible to danger, and I crossed all the crevices without thinking of them. I did not rejoin my two companions till I arrived at the Grand Mulet, from whence we set off for the Glacier des Bossons,* and at half-past eight in the evening, we returned to the Hotel de l'Union at Chamouny, without experiencing any great degree of fatigue. I was the more astonished at this, because, for an hour after the accident, I made great efforts in an elevated situation where the least exertion exhausts the strength.

"I shall here add a few words explanatory of our unfortunate accident. It appeared that the upper stratum of the snow on the acclivity lay upon another stratum, which was very slippery on

* In crossing the Glacier des Bossons, we found a young chamois upon an isle of ice, surrounded with enormous crevices, it had probably died from inanition. One of the high seracs, under the shade of which we had reposed in our ascent, had fallen in the interval, and had covered the spot on which we had stopped with its fragments.

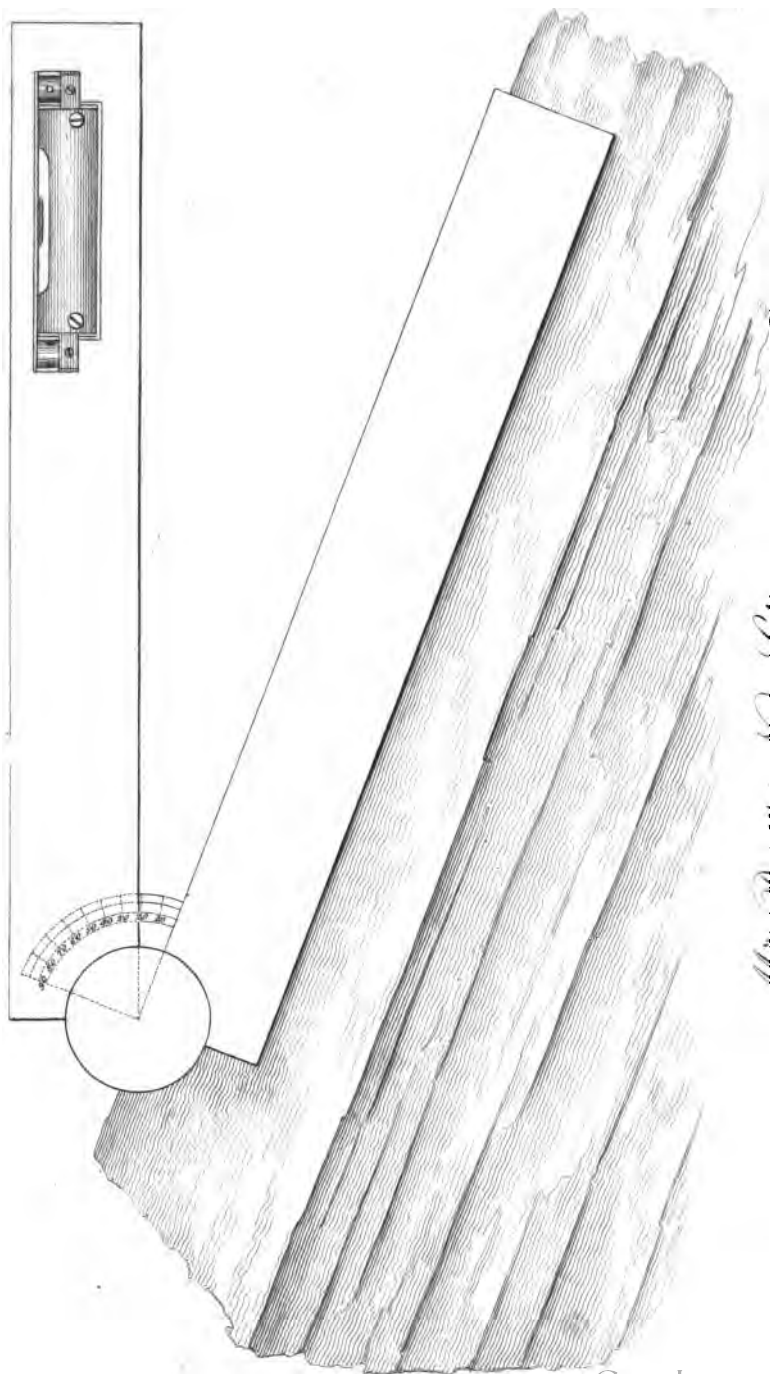
the surface; and as our track cut the first stratum across, the part which was above us began to slip upon the other, forming what is called in the l'Oberland de Berne, *suoggischnee*, or *rutschlavine*. In that part where the first of our file walked, the acclivity was much steeper than near me where I had measured it a little before the accident; there it inclined at an angle of 26° . Further on, the mass of snow was also thicker, especially high up; for the wind usually drifts there the loose snow blown from the summit. For these reasons, the slipping necessarily began at this place, and the snow descended directly towards the crevice; while about me it took an oblique direction forward. It was on this account that the three first of the file* fell so deep into the crevice, and were covered with snow, so that we were unable to discover them, while the fifth and sixth,† who had also fallen in, were able to disengage themselves. Coutet came up with his face of a blue appearance, and with symptoms of suffocation. Mathieu Balmat, who was a very strong man, and one of our principal guides, walked fourth, was the only one who could withstand the slipping of the snow. Thrown down and afterwards carried to some distance, he had the presence of mind to thrust his large stick down, like an anchor, into the hardened snow. The two other guides‡ were, like us three travellers, buried in the snow, and forced towards the crevice, without, however, falling into it. The guides reckoned the surface of the snow which moved to be nearly 100 fathoms broad, and 250 high in an oblique direction. From the firmness of the snow which had slipped, it was evident that it had not lately fallen. The guides most accustomed to the snow did not suspect any danger. At the moment the accident occurred, the brother of one of our principal guides walked first, and the second was a man who had been this journey 12 times. In coming from the side of St. Gervais, passing by the Aiguilles and the Dôme du Gouté, it is necessary to take the route to Chamouny, in order to reach the acclivity, which deceived us when we imagined all dangers were past.

"Whether we ascend one side or the other, even after having escaped as I did, the formidable rocks of the Aiguilles du Gouté, and passed the gulphs of the Glacier des Bossons, we incur the danger near the summit of being swallowed up by the yielding of the snow which at first appears to be firm, but suddenly gives way—a species of danger against which it is difficult to find a preservative."

* They were Pierre Balmat, brother of Mathieu, and eldest son of P. Balmat, one of the ancient guides of M. de Saussure; Pierre Carrier, a smith by trade, who had been 11 times upon Mont Blanc; and Auguste Terraz. This last and P. Balmat had never been on Mont Blanc, and were the two guides who refused to remain at the Grand Mulet. These three carried the provisions, the instruments, the pigeon, and a live fowl. No one of them was married.

† Joseph Marie Coutet, one of our principal guides (his father was also with M. de Saussure), and Julien Devvauxson, who escaped being poisoned by oil of vitriol.

‡ David Coutet, the brother of Joseph Marie, our principal guide, and David Foligue.



Mr. Pratt's New Clinometer

ARTICLE VI.

Description of a new Clinometer. By S. P. Pratt.

(With a Plate.)

(To the Editor of the *Annals of Philosophy*.)

SIR,

Tottenham, Dec. 1820.

I SEND you a drawing (Pl. II.) of a simple instrument which I have had some time in use for determining the dip of strata; it consists of a brass ruler, of 12 inches by $\frac{1}{4}$ of an inch in breadth, closing upon an hinge, and covering, when closed, an arch drawn from the centre of the hinge, and divided into 90 degrees on the same side; near one extremity of the ruler a small spirit level is inserted truly horizontal, with its edge—the application is obvious. The ruler being closed, with the level uppermost, is placed parallel to, or upon any projecting edge of, the strata to be examined, and then gradually opened until the level becomes horizontal; the bisection of the arch near the hinge of the ruler will give the angle formed by its two legs, and consequently the inclination of the strata with the horizon.

I am, Sir, yours,

S. P. PRATT.

Note.—The simplicity of this instrument appears strongly to recommend it. It is made and kept for sale by Mr. Bate, Poultry. *Ed.*

ARTICLE VII.

On Red Snow. By Dr. Henderson.(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Curzon-street, Dec. 9, 1820.

IN the several accounts which were given of the red snow found in Baffin's Bay, I do not recollect that any one has adverted to the fact of this phenomenon being familiar to the ancients. While I was lately turning over the pages of the indefatigable Pliny, for a very different object, I stumbled upon a passage (lib. xi. c. 35) where he mentions that snow turns red by age—"ipsa nix vetustate rubescit." Nor is the cause thus assigned for the colour by any means inconsistent with the best analyses of the red snow as brought to this country in a liquid form; for it is evident that if this variation of the colour of the snow on particular spots be the result of the formation of a lichen or byssus on its surface, it can only be on the old snow that such vegetation will occur. I remain, dear Sir,

Your most obedient servant,

M. HENDERSON.

ARTICLE VIII.

A new Method of drawing a Tangent to the Circle.

By Mr. William Ritchie, AM. of the Academy, Perth.

(To the Editor of the *Annals of Philosophy*.)

SIR,

I HAVE taken the liberty of sending you a new method of drawing a tangent to the circle which appears to me simpler than any of those already known, and which I hope will find a place in your *Annals of Philosophy*. For the advantage of the young geometer, I have accompanied it with the analysis, or the mode, by which it was discovered. I am, Sir,

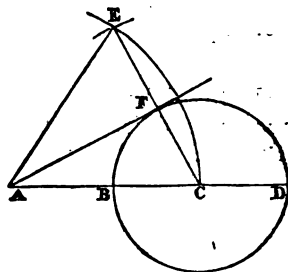
Your obedient servant,

WILLIAM RITCHIE.

Analysis.

Let BFD be the given circle, C its centre, and A the point, from which the tangent is to be drawn; and let AF be the tangent required. Join CF , and produce it till FE be equal to FC , join AE .

Now since the angle AFE is equal to AFC , each of them being a right angle, and since FE is equal to FC , and AF common to the two triangles AFC and AFE , these triangles are equal, and consequently AE is equal to AC , which is given. Again CE is given, being equal to the diameter of the circle; therefore the point E is given, the line CE and F its intersection with the circumference, which is the point required.

*Composition.*

From A with the radius AC describe an arc, and from C with a radius equal to the diameter of the circle describe another, intersecting the former in E ; join CE and F its intersection with the circumference of the circle will be the point of contact required.

For CE being equal to the diameter of the circle, and CF the radius, FE is equal to FC , AC is equal to AE , and AF common to the two triangles AFC and AFE ; therefore, these triangles are equal to each other, and consequently the angle AFE is equal to the angle AFC ; that is, AFC is a right angle. Hence AF is a tangent to the circle.

ARTICLE IX.

On the Dry Rot. By Mr. W. M. Dinsdale.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Nov. 25, 1820.

PERMIT me to ask some of your more scientific correspondents, "In what light they view the following analogous circumstances connected with the subject of dry rot in timber, and whether they do not in some measure consider them to throw a ray of elucidation on that important head?"

1. In recent vegetable juices, I think, we have grounds for believing in the existence of acetous acid as well as its constituents.

2. Casks used for manufacturing vinegar soon decay if they are not thoroughly cleansed from the mother or sediment formed in the operation.

3. Baked wood is less subject to dry rot than that which is unbaked.

4. Tar, both of the Stockholm kind, and that which had been made at the gunpowder charcoal works, has been found to rot the palings at Woolwich, &c. in a space of time seldom exceeding three years, and frequently considerably less, which rot was decidedly attended by all the appearances of dry rot; while it is equally certain the rot alluded to has never followed the use of either kind, when freed from its pyroacetous acid. I am, Sir,

Your obliged and obedient servant,

W. M. DINSDALE.

*** We are in some doubt as to the absolute correctness of our correspondent's views, but as the subject of the dry rot is one of great importance, we shall be happy to insert any hints which may tend to elucidate its causes, or remedy its effects.—ED.

ARTICLE X.

On the Fall of Rain.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Dec. 9, 1820.

THE letter inserted in the last volume of your *Annals of Philosophy*, p. 421, recalled my attention to M. Flauguerges's paper, and more particularly to the remarks which he makes (*Annals*, vol. xiv. p. 113, 114,) on the relative quantities of rain which are

received by gauges at different heights from the surface of the earth. I do not mean to enter into the controversy with respect to the direction of the drops : this I shall leave to Mr. Meikle and his antagonists, but there is another part of the question which seems to have been overlooked, and to which, therefore, I could wish to call the attention of your readers.

I am persuaded by private observations, as well as by those which have been made public, that the fact is certainly true of more rain falling near the earth's surface than at some height above it. This seems to be a paradox, and many have, therefore, endeavoured to explain it away, but still it seems to me to be a necessary consequence of the very constitution of the atmosphere. Clouds collect before rain falls, but this is not in consequence of their being the only source of moisture, but of the upper strata of the air being first affected by those causes which produce the rain. The quantity of rain is made up of a general discharge of water from the air between the earth and the region of the clouds. Hence the effects must accumulate as we approach to the earth, and the results which have been observed must not be wholly attributed to the wind, or to any accidental circumstances which may affect the instruments which are used for the admeasurement.

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ARTICLE XI.

Some Observations on Whale Oil. By J. Bostock, MD. FRS. LS. and HS. MRI. Mem. of the Med., Geol., and Astron. Soc. &c. &c.

Great Coram-street, Dec. 21, 1820.

THE object of this communication is to give an account of some experiments in which I was concerned that were made in order to ascertain the changes that are produced in oil by exposing it for a length of time to an elevated temperature.* Without going into a minute detail of the individual experiments, I shall state some of the most important of the results, and shall afterwards offer a few observations upon the nature of the process and upon the mode by which the change is effected. The quantity of oil operated upon was generally from 25 to 30 gallons; the fluid was contained in a boiler three feet long, one foot six inches wide, one foot three inches deep; fire-place 14 inches long, three inches wide; the bars three inches from the bottom

* The experiments were performed at the manufactory of Messrs. Taylors and Martineau, in the presence of several scientific gentlemen: among others, of Mr. Children, Mr. Aikin, Mr. Daniell, Mr. Richard Phillips, and Mr. Faraday.

of the boiler. The oil occupied about two-thirds of the vessel; it had a concave bottom, and was closed air-tight, except that a tube of half an inch diameter was inserted into its upper part. The temperature employed was 360° , and the oil was carefully kept at that degree of heat during 12 hours each day. In the different trials the process was continued for 22, 23, 26, 38, and 56 days respectively, until, in one instance, it was extended to 68 days. The substance employed was the whale oil of commerce, in the state of purity in which the article is usually exhibited for sale.

When the oil was examined, after having been kept at the above temperature for the periods above stated, its physical properties were considerably altered; its colour was nearly black, its consistence thick and tenacious, and its odour empyreumatic. When heat was applied to it in this state, after it had been cooled down to the temperature of the atmosphere, the first effect was to render it more fluid; and at higher degrees of heat its consistence seemed to be considerably less than that of recent oil. While the oil was in the boiler, and at the temperature of 360° , an internal motion took place among its parts, that seemed to arise from some portion of it being converted to the aeriform state, and suddenly condensed: this was indicated by a peculiar sound emitted from the vessel analogous to the simmering of water before it is raised to the proper boiling point. It was observed that the simmering, if we may so term it, diminished as the oil exceeded the temperature of 400° ; and after it had acquired the heat of about 450° was no longer heard.

But the most material alteration in the oil was the property which it had acquired of emitting vapour when it was subjected to temperatures which would have had no effect of this kind upon recent oil. Both the quantity of the vapour, its chemical composition, the mode of its generation, and the degree of the thermometer at which it first appeared, were very different in the different experiments; and there does not appear to be sufficient data for forming a correct opinion upon any one of these points. One of the circumstances which seemed the most favourable for its production was the rapidity with which the fluid passed through a certain range of temperature. Oil, for example, which was steadily kept at 360° , although it had acquired the dark colour, seemed to produce little aeriform fluid of any kind, with perhaps the exception of a portion of carbonic acid; and there is some reason to suppose that it might bear even a higher temperature, provided the heat be cautiously applied. But if, on the contrary, the heat be more rapidly raised, a copious discharge of an aeriform fluid takes place, which essentially consists of inflammable and aqueous vapour mixed in variable proportions. The total amount of vapour emitted, the proportion of the aqueous to the inflammable part, and the che-

mical nature of this latter, have not been correctly ascertained, nor is it known whether any thing is procured which is entitled to the technical appellation of gas. It may, however, be stated generally, that the quantity of vapour is large, that the proportion of the ingredients vary in different parts of the same experiment, and that the aqueous is more condensable than the inflammable part. This appeared by bringing a cold body in contact with the vapour, and the condensation frequently was observed to take place merely by the ordinary temperature of the atmosphere: in this case the inflammable part was left in a highly combustile state. The difficulty with which this inflammable vapour is condensed was strikingly illustrated by one experiment, in which the oil vapour was passed through a worm tube of 23 feet in length: by this means the aqueous part was entirely removed, and the residue burned with a continuous bright flame. The water collected in this case was strongly acidulous, in consequence, as it appeared, of the copious generation of acetic acid.

In most of the experiments the emission of inflammable vapour was scarcely perceptible below a temperature of 400° ; but in one instance, where the oil, after having been in the boiler for 55 days, was suffered to cool to the temperature of the atmosphere, and then heated by a brisk fire, a quantity of vapour was generated below 210° (the degree at which the graduation of the thermometer commenced), which, by the application of a lighted taper, exploded with some violence. Speaking from general observation it would seem that when the temperature of the oil is raised to about 420° or 430° , the proportion of the inflammable to the aqueous vapour is more considerable. If the temperature be further raised to about 480° , the proportion of the aqueous vapour is increased; while at a still higher temperature, above 500° , the inflammable vapour again predominates. With respect to the phenomena which the mixed vapour exhibits during combustion, it may be remarked that its inflammation is not attended with detonation; when the aqueous part prevails, the flame is quickly extinguished; but that in proportion as it is freed from the aqueous vapour, it burns with a considerably dense and brilliant flame.

Another effect which appeared to result from the long continued application of heat was, that the oil had its boiling point lowered, or, to speak more correctly, that below the temperature which is generally regarded as the boiling point of recent oil, the heated oil was rapidly converted into an inflammable vapour, but of a different kind from that procured at lower temperatures; the vapour procured from the boiling oil containing less water, and having a highly offensive and most penetrating odour; whereas the former vapour had comparatively little smell, and that not peculiarly disagreeable. The change to

which I now refer, generally took place at about 580° .* it appeared to consist in the vaporization of the residual fluid in the boiler; but it was not strictly entitled to the appellation of boiling, because it does not appear that by condensing the vapour, a fluid could be reproduced similar to that from which it was procured.

The above observations are to be regarded as matters of fact, independent of hypothesis; but it is impossible to contemplate the phenomena without speculating upon the changes which the oil may be supposed to have undergone by the long continued action of heat upon it. We may conceive that during this operation the elements of the oil act upon each other, and that the fluid is converted into an heterogeneous compound; that the first new product is a substance containing a larger proportion of hydrogen, and constituting a highly volatile oil, capable of being brought to the aeriform state by a comparatively low temperature. The next accession of heat seems to be attended with the production of water, which is emitted along with the vapour of the volatile oil; while at length, by a further increase of temperature, the substance remaining in the boiler, now deprived of a considerable proportion of its oxygen and hydrogen, gives rise to a highly inflammable pungent vapour, comparatively free from water, but accompanied by a considerable proportion of acid. Although the experiments seem to prove that at least a small quantity of the volatile oil is capable of being generated at the temperature of 360° , yet it is probable that the chief effect of this temperature is to produce a change in the oil, which may dispose it to the production of the volatile matter at a somewhat greater heat, the mode in which the heat is applied being, to a certain extent, more important than the actual degree to which it is carried.

It is unnecessary to remark that the facts which have been observed, although, as I conceive, new and important, are to be considered as constituting only the first step of an investigation, which will probably lead to many curious results. In what degree the phenomena depend upon any thing peculiar in the nature of whale oil in its ordinary state of purity, or upon any heterogeneous substance contained in it, is the first point that it will be desirable to ascertain. In the next place it will be important to watch the gradual progression by which the oil has its physical and chemical properties so much changed, and to discover at what degree, or after what period of time, the maxi-

* In one of our experiments a species of ebullition occurred at a much lower temperature, about 460° , the fluid being violently projected in considerable jets from the vent pipe to the height of nine feet or more, when the surface of the oil was about five inches below the cover of the boiler, and the orifice of the vent pipe screwed into it. We were, however, induced to regard this as not the result of the rapid volatilization of the entire fluid, but as depending upon the more volatile part of it being suddenly converted into vapour, which, intimately mixing with the viscid mass, forced a portion of it out of the tube.

num of effect takes place, or whether indeed there be any term of this kind. It will be next desirable to learn what is the lowest temperature at which the volatile oil and the water are respectively generated, and to examine at different temperatures, and at different periods of time, the proportion which these substances bear to each other. It will likewise become a very curious subject of investigation to learn with more accuracy the nature and properties of the inflammable vapour, whether it consist merely of the volatile oil in a vaporized state, or of this mixed with some permanent gas; if so, whether the gas resemble any species of carburetted hydrogen with which we were previously acquainted. Lastly, we must investigate the nature of the volatile oil, when obtained in a separate state by distillation, and also that of the acid generated in the latter part of the operation, whether it be actually the acetic acid, or in what respect it differs from this acid as obtained by the ordinary processes.

ARTICLE XII.

On the Preparation of pure Salts of Manganese, and on the Composition of its Oxides. By G. Forchhammer, Ph. D.

THE principal ore of manganese wrought for the preparation of chlorine is the common grey ore of this metal, or the peroxide. I have tried several of the ores which occur in Germany in veins and beds in a porphyry belonging to the first secondary sandstone, and found them all containing some copper, in very small quantity, excepting of course the complete crystals of it which are generally free from this admixture. It appears that the Devonshire ore is found in the same geognostic position as those above-mentioned; and I found this likewise containing some traces of copper, which, though the combination does not bear the character of a chemical compound, is a striking instance of the similarity in the formation of the same mineral in far distant countries. Besides copper, iron always is present, and a small quantity of barytes, and even of lead, occur very often. Iron, however, is an admixture, the separation of which has generally been the most attended to; and many chemists have attempted to remove it by simple processes. There is no doubt that the benzoate and succinate of any alkali will throw down all the deutoxide of iron; but if there is protoxide, it may escape to a certain degree, and then these salts are so expensive that, for preparing a quantity of the pure oxide of manganese, it would be desirable to find another process. The method I made use of is the following: I prepared in the common way sulphate of manganese by heating equal parts of peroxide of manganese with sulphuric acid, only taking care to keep it in the fire until vapour of sulphuric acid ceased to appear;

by these means the bisulphate of protoxide and the sulphate of deutoxide are decomposed, and converted into sulphate of protoxide. The solution, which of course could contain neither barytes nor lead, whatever quantity of them there might be in the ore, contains only the sulphates of manganese, iron, and copper. In order to remove the two latter metals, I poured successively into the solution of the manganese a solution of hydrosulphuret of ammonia, which first precipitates the copper of a black colour, then the iron likewise black, and at last the manganese of a white colour. When the colour of the precipitate turns grey, I heat the liquor to the boiling point, and then allow it to remain undisturbed for the purpose of trying it with tests.

The prussiate of potash is sufficient for that purpose, which precipitates the salts of pure manganese of a white colour; the slightest trace of iron is directly shown by a blue colour, and copper by red.

The crystals of sulphate of manganese which I made use of for the analysis were made in a different way to avoid the presence of any alkali. Through the impure sulphuric solution of manganese, I passed sulphuretted hydrogen to remove the copper, and then concentrated it by evaporation until it became a very strong solution, which, however, did not crystallize when cold. Spirit of wine containing 80 to 90 per cent. of alcohol divides the solution into two parts, the lower of which soon deposits crystals of sulphate of manganese, often entirely free from iron, but sometimes they contain a very slight portion of it.

From the pure sulphate of manganese above-mentioned, I obtained by carbonate of potash the carbonate of manganese, which yielded me all the different oxides.

I tried often to prepare the pure metal from one of these oxides, but I never could succeed. I exposed it, with charcoal, to the heat of a furnace, in the china manufactory in Copenhagen, where iron melts very easily; but though the oxide was reduced, and the powder in the crucible dissolved in acids, with the evolution of much hydrogen, it was not melted; and whether it was metal, or an oxide containing less oxygen than the protoxide, I was not able to ascertain. In another experiment, when I exposed it to the heat of a large anchor-forge, I obtained small grains.

On the Protoxide.

I found it rather difficult to obtain a protoxide completely free from all deutoxide. The usual way, by heating carbonate of manganese in a retort full of carbonic acid, did not appear to me very accurate, as I never was able to obtain a carbonate of protoxide of manganese completely free from all deutoxide. All the solutions of protoxide, if they have been exposed to the air,

contain a small quantity of deutoxide which occasions their reddish colour, and the carbonate being obtained from them of course contains the same. That the red colour of the solutions of manganese depends upon the presence of deutoxide, I had an opportunity of observing in the following way :

In my paper on the Acids of Manganese, I have mentioned the mode in which I decomposed the brown compound of lead, manganese, and oxygen, by bisulphate of potash ; and I made use of the same method for ascertaining the quantity of oxygen combined with protoxide in the deutoxide and peroxide. I found that when the greater part of the oxygen was expelled by the combined action of heat and the bisulphate of potash, the powder was entirely dissolved, and formed a beautiful transparent amethystine coloured solution, which still yielded oxygen, and became of a pink colour. The expulsion of oxygen did not cease until the melted salt in the retort was entirely colourless, and it remained so when cold. A solution of it in water was colourless, but, when exposed to the air, it soon acquired the slight pink colour, which is characteristic of all the salts of manganese. This experiment proves obviously that the colour depends upon the surplus of oxygen combined with the protoxide, or that all these pink-coloured solutions and salts contain both protoxide and deutoxide ; the latter, however, only in very small quantity.

The way I proceeded in order to obtain pure protoxide was this : I filled a glass tube, open at both ends, with deutoxide, and heated it over a lamp while I passed hydrogen gas through it. The brown powder soon changed to a light yellow, which colour, while the powder was cooling, became white, and the cold oxide was of a beautiful light-green. This colour, however, changed in the air ; for while I was taking the open tube to a pair of scales in another room, it evidently attracted oxygen, and its colour changed into greyish-green ; and when converted by combustion again into deutoxide, it gained five per cent.

There is not any reason to induce us to suppose that in this case the protoxide had been reduced to suboxide by the action of hydrogen, for the heat was very gentle, and from the power with which the metal decomposes water even in the common temperature of the atmosphere, we scarcely can expect that in so slight a heat, hydrogen should deprive the protoxide of a part of its oxygen.

To ascertain the quantity of oxygen in the protoxide, I analyzed the sulphate and carbonate.

I heated a certain quantity of sulphate of manganese to redness, then dissolved it, and precipitated the sulphuric acid by nitrate of barytes. Five grammes of perfectly dry sulphate of manganese gave me 7.913 grammes of sulphate of barytes,

which are equivalent to 2·7181 grammes of sulphuric acid, allowing that 100 parts sulphate of barytes contain 34·35 parts of sulphuric acid.

One hundred parts of sulphate of manganese consist, therefore, of

Sulphuric acid.	54·378
Protoxide of manganese.	45·622
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	100·000

One hundred parts of protoxide of manganese combine with 119·192 parts of sulphuric acid, which contain 71·5155 parts of oxygen, and thus we find the protoxide of manganese consisting of

Oxygen.	23·8385
Manganese.	76·1615
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	100·0000

Or 100 parts of manganese combine with 31·29 parts of oxygen to form the protoxide.

In another experiment, where I had reason to suspect that the heated sulphate of manganese had again attracted some water, I obtained 7·731 grammes sulphate of barytes from five grammes of sulphate of manganese. After having precipitated all the excess of barytes in the solution, I threw down the manganese by carbonate of potash, and having converted it into deutoxide by exposing it to a red heat in an open vessel, the quantity obtained was 2·480 grms. I shall be able hereafter to prove, that the deutoxide consists of 92·4342 parts of protoxide and 7·5658 parts of oxygen: 2·480 grms. of deutoxide consist, therefore, of

Oxygen.	0·18764
Protoxide.	2·29236

The sulphate of barytes obtained indicates 2·6555 grms. sulphuric acid, which combine with 2·29236 parts of sulphuric acid.

One hundred parts of protoxide of manganese, according to this experiment, would combine with 115·90 of sulphuric acid; and they, therefore, would consist of

Oxygen.	23·18
Manganese.	76·82

Or 100 parts of manganese combine with 30·18 parts of oxygen to form the protoxide.

I several times heated the deutoxide with sulphuric acid; and by the quantity of sulphate of manganese thus obtained, I endeavoured to find the quantity of sulphuric acid, but the vapours of this acid always carried off some sulphate of manganese, which was

again deposited on the cover of the crucible, and partly lost. The smallest quantity of sulphuric acid which, in that way, I found combined with 100 parts of protoxide, was 113 parts, which indicates 29.19 parts of oxygen as combined with 100 parts of manganese in the protoxide. Berzelius finds only 28.10, which certainly is too little.

From the purest sulphate of manganese, I prepared carbonate of manganese, avoiding oxidation as much as possible; five grammes exposed during a long time to a red heat in an open vessel yielded 2.803 parts of deutoxide = 2.58775 protoxide.

Two decigrammes of the carbonate yielded by muriatic acid in a tube with mercury 2.175 French cubical inches of carbonic acid. The weight of the carbonic acid at a medium temperature and pressure is 0.0661 grm. and the salt contains 33.05 per cent. of carbonic acid: 33.05 parts of carbonic acid contain 24.036 parts of oxygen; and, therefore, the 2.58775 parts of protoxide of manganese contain 0.6009 part of oxygen.

One hundred parts of protoxide contain, according to this experiment,

Oxygen	23.22
Manganese	76.78
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	100.00

Or, 100 parts of manganese combine with 30.24 parts of oxygen.

The composition of the artificial carbonate of manganese is, therefore,

1 atom of protoxide	51.755	{	39.737 manganese
			12.018 oxygen
1 atom of carbonic acid. 33.05		{	9.014 carbon
			24.036 oxygen
1 atom of water.	13.520	{	1.502 hydrogen
			12.018 oxygen
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	98.325		

There was a loss of 1.675, which I imagine consisted of water not chemically combined with the salt.

Dr. John, of Berlin, from 150 grammes of crystallized sulphate of manganese obtained 148.5 grms. of sulphate of barytes, and 83.25 carbonate of manganese, which, when calculated after my analyses of the carbonate, indicates 100 parts of protoxide and 118.43 parts of sulphuric acid, and 100 parts of manganese combined with 31.03 oxygen, to form the protoxide. It seems, therefore, as if some number between 30.18 and 31.29 is the true number for the oxygen combined with 100 manganese in the protoxide.

On the Deutoxide.

The colour of the deutoxide is brown, resembling that of

deutoxide of iron, but rather darker. It is soluble in some acids. Muriatic acid dissolves it when at about 42° Fahrenheit. It is necessary that the muriatic acid should be very strong, and the deutoxide of manganese added in small quantities at a time, in order to avoid increasing the heat of the liquid. Thus a dark-brown solution is obtained, which, in a temperature below 41° Fahrenheit, may be kept a few days unaltered; but when exposed to the sunbeams, or to any increase of temperature, chlorine is evolved, and muriate of protoxide formed. In those cases where I succeeded in charging the concentrated muriatic acid with a considerable quantity of oxide of manganese, the proportions seemed to me to be such that when I destroyed the compound by heat, I obtained chlorine and neutral muriate of protoxide: the experiments, however, on which I found that opinion were not very accurate. When the muriatic solution is poured into a large quantity of water, a mixture of deutoxide and peroxide is thrown down, and muriate of protoxide remains dissolved. The muriatic solution of deutoxide, even when no chlorine can be discovered by the smell, acts nearly in the same way as an aqueous solution of chlorine, metals are instantly dissolved, and an adequate portion of deutoxide brought to the state of protoxide. Tartaric acid, when thrown into such a solution, instantly emits carbonic acid gas; and sulphurous acid and sulphuretted hydrogen, when in contact with it, instantly form sulphuric acid.

The sulphuric acid acts nearly in the same way as the muriatic acid, and the sulphuric solution of deutoxide has the same properties with respect to metals and vegetable substances as the muriatic solution, which induced Mr. Giobert, about 30 years ago, to believe in the existence of an oxygenated sulphuric acid. Sulphuric acid, when much diluted, decomposes the deutoxide and forms peroxide and sulphate of protoxide. I never could obtain a nitric solution of deutoxide, but always procured nitrate of protoxide and peroxide.

It is a curious fact that the existence of this deutoxide has been very often denied, though it is of all the different oxides of manganese, the only one which is easily obtained in its pure state. As the quantity of oxygen found in this oxide seemed not to agree with the rules laid down for the relation of the oxygen of the different oxides, it was, in contradiction to all its properties, supposed to be a compound of two oxides; viz. of the protoxide, and a deutoxide which does not exist. This oxide is formed by the combustion of the protoxide of manganese in the oxygen of atmospheric air, as was observed by Scheele. When pure protoxide is heated in the air between the boiling point of water and that of mercury, it suddenly begins to take fire, and burns slowly with a reddish light. This only takes place when the protoxide does not contain carbonic acid. The carbonate, when heated to the same temperature, and under the

same circumstances, is converted into peroxide without any visible fire. I believe the single circumstance that the brown deutoxide is the result of combustion without the interference of any other substance is sufficient to prove it to be a real oxide, and not a compound of two different oxides. It would be sufficient in my opinion even if the quantity of oxygen found in this deutoxide did not at all agree with that of any of the other oxides. I shall prove, however, that it does agree.

The quantity of oxygen in the different oxides of manganese has generally been ascertained by synthesis, and the results have been very different indeed. I tried the analytical method, and at least found the results consistent with each other, though differing from those of other chemists. I have already described the manner in which I conducted the experiment with bisulphate of potash; I shall, therefore, here only mention the result.

Two grammes of brown deutoxide gave me a bulk of oxygen, which, reduced to the mean temperature and pressure of air, is equal to 5.3243 French cubic inches of oxygen, whose weight is 0.151316603 gramme, and 100 parts of deutoxide consist of

Oxygen	7.5658
Protoxide of manganese	92.4342
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	100.0000

92.4342 parts of protoxide contain 70.403 parts of metal and 22.0312 parts of oxygen; and it is obvious that 22.0312 bears very near the same proportion to (22.0312 + 7.5658) = 29.597 as 3 : 4, which expresses the relative quantities of oxygen in the protoxide and deutoxide.

The composition of the deutoxide is :

Oxygen	29.597
Manganese,	70.403
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	100.000

One hundred parts of manganese combine with 42.040 parts of oxygen in order to form deutoxide.

On the Peroxide.

When the deutoxide of manganese is boiled with pure weak nitric acid, a part of the oxygen is accumulated in one part of the manganese, while the rest reduced to protoxide is dissolved. The peroxide thus obtained is black : it is insoluble in acids and alkalies. Acids have no effect whatsoever on it, unless they are capable of depriving it of a part of its oxygen. Alkalies, when heated slightly with it, accumulate the oxygen in one part, and dissolve the manganeseous acid thus formed; while the other part reduced to deutoxide remains undissolved. This artificial peroxide is a conductor of electricity, and acts in a galvanic

pile nearly in the same way as the grey ore of manganese, which, when perfectly pure, is in its chemical properties exactly similar to it. The peroxide which I obtained by the exposure of carbonate of manganese to the air at a temperature of about 500° Fahrenheit seemed to be exactly the same, when purified by weak cold muriatic acid, which only serves to dissolve the carbonate that might have escaped the action of the heat and air.

Ten grammes of deutoxide, when boiled with nitric acid, gave 4.793 of peroxide, which contained a considerable quantity of water; two grammes, in another experiment, gave 0.935; and one gramme of the first hyperoxide gave, after a long exposure to a red-heat, 0.71 of brown deutoxide; 4.793 would, therefore, have yielded 3.40303, which consist of 0.258466 oxygen and 3.144564 peroxide of manganese; but as 10 grammes of deutoxide lose 0.75658 gramme of oxygen when reduced to protoxide, this oxygen must be combined with 3.14456 grms. of protoxide, in order to form peroxide: 3.14456 grms. protoxide contain 0.74862 grm. of oxygen, and then take up 0.75658 grm. of oxygen to form peroxide. It is obvious, therefore, that the peroxide contains twice as much oxygen as the protoxide, and it is likewise obvious that the peroxide which I made use of for the analysis contained a small quantity of deutoxide, and that instead of 3.40303 grms. of deutoxide obtained from the peroxide, I ought only to have got 3.3333 grms.

The composition of the peroxide is, therefore,

Oxygen.....	36.351
Manganese.....	63.749
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	100.000

and the artificial peroxide is a hydrate consisting of

Oxygen.....	16
Manganese.....	30
Water.....	9

The analysis by bisulphate of potash gave a result which differs somewhat from the above-mentioned: for one gramme yielded five French cubic inches, equal to 0.1421 gramme of oxygen; while I ought to have got 0.1579; and I know of no other way to account for it than by the loss necessarily produced by some fibres of the paper on which the peroxide was collected. The peroxide adheres very much to the filtering paper, and cannot be removed without difficulty; and any combustible matter introduced into the retort with the oxide of manganese would of course produce a great loss of oxygen.

There is a kind of oxide produced by the action of moderate heat on the nitrate of manganese, its colour is blackish-brown, it dissolves in strong muriatic acid with some effervescence.

Berzelius takes it for the real deutoxide; but if we compare the very compound action of nitric acid with the simple effect of combustion in atmospheric air, there scarcely can remain any doubt, that not this, but the first mentioned is the real deutoxide.

Two grammes were by a strong red-heat reduced to brown deutoxide, and lost by that 0.061 grm. This oxide, therefore, consists of

One atom of peroxide . 22.323	{ 2.904 oxygen
One atom of deutoxide 77.677	{ 19.419 deutoxide
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	100.000

ARTICLE XIII.

ANALYSES OF BOOKS.

Pharmacopœia Collegii Medicorum Edinburgensis. 1817.

ALTHOUGH this edition of the Edinburgh Pharmacopœia has been published for more than three years, yet I have been prevented by various causes from offering the observations and experiments which I have made respecting it, until it may appear quite out of date to notice it at all.

I am perfectly aware of the distinguished stations in science which many of the Fellows of the Edinburgh College occupy; and it is requisite only to remind the reader that Dr. Hope is the President, and Dr. Duncan, jun. one of its Fellows, to render it certain that no work of this nature can have appeared without having excited and ensured their unremitting attention.

The remarks which I now propose to offer upon this work will be principally confined to the chemical part of it, occasionally making observations upon the nomenclature; but the points of minor importance, such as the arrangement and the regulations adopted as to weights, I shall leave untouched.

The preparation which I shall first notice is termed *acidum aceticum forte*; it is prepared by distilling a mixture of 12 parts of sulphate of iron, which has been dried to whiteness, with 10 parts of acetate of lead in a glass retort.

The first objection which I shall state to this process is, that the quantities of the salts employed are not such as are required for mutual decomposition. Ten parts of acetate of lead require only about 7.5 parts even of crystallized sulphate of iron for their decomposition, and of this quantity nearly 3.5 are water; admitting that only three-fourths of this proportion of water are dissipated by drying, the 12 parts of dried sulphate of iron will be nearly equal to 18 crystallized instead of 7.5 only as required. Sulphate of iron is not certainly a costly article, but the great

excess renders the use of large retorts necessary, and thus greatly increases the expense of the operation.

The acetic acid actually produced scarcely merits the name of "strong" by which it is described. I find that its strength is to that of distilled vinegar only about as three to one; added to these objections, its odour is extremely unpleasant arising from empyreuma.

Another circumstance to be noticed is, that there is left in the retort a mixture of sulphate of lead and peroxide of iron, from which it is very difficult to clear it for a second operation.

These difficulties are all easily remedied. Let the acetate of lead be decomposed by sulphate of soda; the sulphate of lead being rejected, crystallize the acetate of soda, and decompose it by sulphuric acid. The acid thus obtained is not at all empyreumatic; the sulphate of soda left in the retort is easily dissolved, and serves for a second decomposition; the quantity of sulphuric acid required is only 2.6 parts instead of the 6.4 parts contained in the sulphate of iron directed. It is indeed true that there is the additional process of crystallizing the acetate of soda, but this is more than compensated for by omitting the preparation and drying of the sulphate of iron.

Acidum Muriaticum.—Equal quantities of sulphuric acid and common salt are directed to be employed in the preparation of this acid. It will be seen by Dr. Wollaston's scale that the requisite proportions are 84 parts of acid to 100 of salt.

Acidum Nitrosum.—This acid is directed to be distilled by decomposing 24 parts of nitre by 16 of sulphuric acid; and it may be remarked that the product is not strictly speaking nitrous acid, but a mixture of it and nitric acid, the latter being unquestionably in much the larger proportion.

Without denying the propriety of directing two processes for obtaining what I must consider as really and essentially nitric acid, I think the utility of the present directions is more than questionable. Dr. Wollaston has shown that, to obtain the whole of the acid from nitre in the state of greatest concentration, it is requisite to employ two portions or atoms of sulphuric acid with one of nitre. If, however, as I have observed in my remarks upon the London Pharmacopœia, one portion only of sulphuric acid be employed, that portion of nitric acid, which would otherwise be lost, may be condensed by passing through water.

Considering Dr. Wollaston's statement, and my own on experiments on the subject, I am of opinion that nitric acid ought to be prepared either by the use of one atom of sulphuric acid, or two atoms. The Edinburgh College have steered exactly between these points; but their reason for so doing, I confess, is not to me at all evident.

I put into a retort 24 parts of nitre and 16 of sulphuric acid, and carried on the distillation as long as nitric acid was produced.

The product was of a straw colour, evidently containing but very little nitrous acid; and its specific gravity was 1513, instead of 1520; as stated in the Pharmacopœia. It weighed 11·5 parts; whereas 24 parts of nitre are capable of yielding 17 parts of acid, provided sufficient sulphuric acid is employed to afford water enough to condense the nitric acid. That this is the case is proved by the experiment I am now relating. Sixteen parts of sulphuric acid contain three parts of water; and it will be seen by Dr. Wollaston's scale that these are sufficient to condense only 8·8 parts of dry nitric acid into 11·8 of nitric acid, of specific gravity 15; the difference between actual and theoretical product being only 0·3 of a part, which is as little as can well be expected in experiments of this nature.

I have already observed that the acid which I obtained has only a straw colour, instead of a red one, as the College seem to expect; and I believe that whenever the acid has this red colour, it is owing to the presence of common salt in the nitre, the chlorine of which partially decomposes the nitric acid.

Acidum Nitricum is directed to be prepared by heating nitrous acid, until the reddish part of it is expelled. If pure nitre be made use of in the first preparation, this operation is totally useless: indeed I am at a loss to imagine any utility in it, even if the acid were as red as the College seem to expect. I have on other occasions remarked, that when the red acid is diluted with water (which it always is before it is used either internally, or as a solvent), that the diluted acid is quite pale. If, however, the College wish for pale nitric acid, it would certainly be better to adopt the London process of using equal weights of nitre and sulphuric acid: this method, although too expensive for common purposes, saves the operation of heating the acid afterwards—an operation attended with some loss, and nearly as much risk as the first preparation of the acid. Added to this, unless the nitric acid be condensed by water, when less than two atoms of sulphuric acid are used to one of nitre, there is a great loss of nitric acid, amounting in the process adopted to about $5\frac{1}{4}$ parts out of 17.

Aqua Potassæ.—To prepare this, six parts of carbonate of potash are to be rendered caustic by eight parts of lime. The theoretical quantity of lime is only $2\frac{1}{4}$ parts; four parts, therefore, instead of eight would have been more than sufficient. The excess occasions waste by the quantity of solution retained.

Subcarbonas Ammonia.—One part of muriate of ammonia is directed to be decomposed by two parts of carbonate of lime. The equivalent quantities are nearly 94 of the carbonate to 100 of the muriate; it must, therefore, be admitted that one part and a half would be sufficient instead of two. The excess requires the use of larger vessels and more firing.

Aqua Ammonia.—In this preparation, the quantity of lime is uselessly and inconveniently large. Muriate of ammonia is

decomposable by very little more than half its weight of lime: the quantity ordered is once and a half its weight. I have tried the process, and found it extremely inconvenient; it is a most pungent operation to mix, as here directed, lime and muriate of ammonia, the quantity of lime being so very large, much delays the getting it into the retort, to the great annoyance of the operator. I confess I prefer the modification of the London process of 1787, which I have recommended, and which, with a slight variation, the London College have now adopted. I found the specific gravity of the aqua ammoniæ of the Edinburgh Pharmacopœia to be 0.936, agreeing very nearly with their statement; viz. 0.939.

Estimating by the strength of the products merely, the process of the Edinburgh College is more economical than the London in the proportion of 16 to 10. As, however, the former order the large quantity of lime, which they employ with the muriate of ammonia, to be made red-hot, the retort generally breaks; and as this does not follow as a matter of course in the London process, I consider it to be really most economical; and having tried both, it is certainly more easily managed.

Tartras Antimonii, incorrectly so called, instead of *Tartras Antimonii et Potassæ*. This salt is directed to be prepared by the old mode of boiling crocus of antimony in a solution of tartar. The objections to this process I have stated at length in my remarks on the London Pharmacopœia. I shall, therefore, only repeat, that it is an extremely inconvenient one on account of the difficult solubility of the crocus, unless it has gone through the troublesome process of levigation or elutriation.

Carbonas Ferri Præcipitatus.—Four parts of sulphate of iron are directed to be decomposed by five parts of carbonate of soda. Scarcely $4\frac{1}{2}$ parts of the alkaline salt are requisite.

Tartras Potassæ et Ferri.—This salt is correctly named; whereas, as already observed, the potash has been omitted in the chemical name for tartarized antimony.

Acetas Hydrargyri.—This salt, which is not contained in the London Pharmacopœia, is directed to be prepared by dissolving three parts of mercury in four and a half of diluted nitric acid, or in a little more than is required to dissolve it. This solution is to be decomposed by three parts of acetate of potash dissolved in about 42 times its weight of water.

It is of importance in making this salt, that the mercury should be as nearly as possible in the state of protoxide, otherwise but little acetate of mercury will be obtained; for if the peracetate be formed, it is a very soluble salt, and protoacetate only is deposited in crystals.

On this account I do not see why the College has ordered rather more nitric acid than is required to dissolve the mercury. The excess, which in this case amounts to about one-ninth part, is extremely liable to be decomposed by, and to convert the

mercury into, peroxide, especially if heat be used; and then the loss which I have described will ensue. It is quite requisite that the acetate of potash should be pure; if it contain any sulphuric or muriatic salts, the protoxide of mercury will be thrown down by them. The salt which I obtained was formed by using acetate of soda, not finding the acetate of potash, usually met with, sufficiently pure. Seventy-two parts of mercury yielded me only 29 of acetate. Upon adding muriate of soda to the remaining solution, I obtained some calomel, showing that the solution should be evaporated for the further production of acetate. After this, I obtained by the addition of soda a large quantity of peroxide of mercury, as will be readily conceived when it is observed that 72 parts of mercury gave only 29 of acetate. A part of this loss I am inclined to attribute to the excess of nitric acid already noticed. It appears to me that acetate of lead, but I have not tried it, might be advantageously used instead of acetate of potash. It must certainly be free from sulphuric and muriatic salts. After what I have stated I think it is evident, first, that less nitric acid should be used; secondly, that less water should be employed, or the solution evaporated to obtain more acetate; thirdly, that the peroxide formed, should be obtained by precipitation for preparing the oxidum hydrargyri rubrum.

Murias Hydrargyri Corrosivus.—Two parts of mercury are directed to be boiled with two parts and a half of sulphuric acid, and the sulphate of mercury resulting is to be decomposed by four parts of common salt.

I have already noticed, that in preparing muriatic acid, the College order common salt to be decomposed by an equal weight of sulphuric acid, a quantity unnecessarily large. In this instance, it appears to be supposed that the remaining portion of two parts and a half of sulphuric acid which the sulphate of mercury contains are capable of decomposing four parts of common salt. It is probable, but I do not speak with certainty, that at least 2.75 parts of the four parts of salt are in excess, and totally useless.

Submurias Hydrargyri Precipitatus.—In order to prepare this, equal weights of diluted nitrous acid and mercury are to be used to obtain a solution of protonitrate of mercury, which is to be decomposed by muriate of soda. The quantities of acid and mercury appear to be perfectly well apportioned, scarcely an atom of mercury remaining undissolved. In this case, as well as in that of preparing the acetate of mercury, it is of great importance that the mercury should not be converted into peroxide; but the directions are singularly inconsistent with each other. In preparing the nitrate for the acetate, more acid is directed to be used than is required for dissolving the mercury. In preparing the nitrate for the submuriate, the orders are with more propriety directly reversed, more mercury being directed to be added to the acid than it is capable of dissolving.

Even however, when more mercury is used than the acid can dissolve, a large portion of peroxide of mercury is formed, which may be thrown down by an alkali after the precipitation of the calomel, I found that 48 parts of mercury gave 15 of peroxide after the calomel had been formed and separated.

Oxidum Hydrargyri Cinereum.—Two hundred and forty parts of calomel are directed to be decomposed by as much lime-water as contains about 180 parts of lime. Now the muriatic acid yielded by the calomel employed cannot combine with more than 30 parts of lime : I do not mean to say, if lime-water be used, that an excess is of no importance ; but I think that in the quantity used, a portion must during ebullition be converted into carbonate, and mix with the oxide of mercury obtained. It seems to me that potash is a much better alkali than lime for this purpose ; being so readily soluble in water, a small quantity of the solution is sufficient.

Oxidum Hydrargyri Rubrum per Acidum Nitricum.—This substance, to which so intolerably long a name has been given, is prepared by first dissolving three parts of mercury in four of dilute nitrous acid. As I have already noticed that mercury is soluble in its own weight of this acid, and that some peroxide is formed ; it appears, therefore, that one-fourth more acid is now ordered than is necessary. The decomposition of that nitric acid which holds protoxide of mercury in solution is sufficient at a high temperature to convert it into peroxide.

Subsulphas Hydrargyri Flavus is formed by boiling together two parts of mercury and three of sulphuric acid. In preparing corrosive sublimate, the proportions are two of metal to two and a half of acid. In one case the acid must be redundant, or in the other, deficient.

Acetas Plumbi.—Prepared by dissolving white oxide of lead in distilled vinegar. By white oxide of lead is meant, I suppose, carbonate of lead, no white oxide existing, or being mentioned among the materia medica. Litharge is the oxide usually employed by the manufacturers of sugar of lead.

It is of little importance to notice this preparation, because, while it can be bought, prepared as it now is in purity on the large scale, it will always be preferred on account of its cheapness.

In concluding these observations, I wish to remark, that although I have scarcely mentioned any processes of this work with approbation, it is not because I have found them unworthy of it : on the contrary, there are many parts which I can recommend with the greatest confidence to those who may in future undertake the office of reforming a Pharmacopœia.

ARTICLE XIV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Nov. 23.—A letter to the President from the Earl of Morton was read, communicating a curious fact in natural history.

The Earl of Morton had received some years since a male Quagga from the Cape of Good Hope. From this and an Arabian blood chestnut mare a female hybrid was produced, which possessed indications both as to colour and form of her mixed breed. In the possession of another person, the same mare was afterwards bred from by an Arabian horse; the result was singular, as, although it had the general appearance of the Arabian breed, it bore, in several respects, characters which belong to the Quagga, such as stripes and marks on the body, and in the hair of the mane.

Nov. 30.—This being the usual day for the election of officers for the ensuing year, the following noblemen and gentlemen were appointed:

President.—Sir Humphry Davy, Bart.

Treasurer.—Davies Gilbert, Esq. MP.

Secretaries.—William Thomas Brande, Esq.
Taylor Combe, Esq.

Council.—Dr. Ash.

William Blake, Esq. MA.

Earl Brownlow.

Bishop of Carlisle.

J. G. Children, Esq.

Sir G. Clerk, Bart.

H. T. Colebrooke, Esq.

J. W. Croker, Esq.

Charles Hatchett, Esq.

J. F. W. Herschell, Esq.

Sir Everard Home, Bart.

Capt. Henry Kater.

J. Planta, Esq.

J. Pond, Esq.

Earl Spencer.

Dr. W. H. Wollaston.

Dr. T. Young, Foreign Secretary.

Dec. 7.—Sir Humphry Davy, on taking the chair, in a short address adverted to the objects of the Royal Society: he alluded also to those philosophical associations whose objects were similar, but confined to particular branches of science. The present state of the sciences, and the great share which the Royal Society had in their improvement, were next pointed

ent; and, as connected with chemistry, he recommended the subjects of fluorine, and the amalgamation of ammonia with mercury.

Dec. 14.—A paper, by Mr. Faraday (communicated by the Secretary), was read. It described the composition and properties of two new compounds of chlorine and carbon, and one of iodine, hydrogen, and carbon. The reading of this paper was concluded on the 21st, when the Society adjourned.

The first chloride of carbon obtained by Mr. Faraday was procured by acting upon chloric ether with chlorine. The muriatic acid gas formed by their mutual action, when exposed to the sun's rays, being repeatedly expelled by the introduction of fresh chlorine, a crystalline substance was obtained, which is the perchloride of carbon. Mr. Faraday then details a method by which it may be obtained in purity, and it then has the following properties: It is colourless and transparent: its smell is rather aromatic, somewhat resembling that of camphor. Its taste is very slight; and it is nearly twice as heavy as water. It is easily pulverized; when scratched, it has the appearance of white sugar, and it is of about the same degree of hardness as this substance. It is a non-conductor of electricity. At a medium temperature, it volatilized slowly; at 320° it melts; and at 360° it boils. It is insoluble in water, but soluble in alcohol and in ether; it is not readily combustible under common circumstances; but heated in pure oxygen gas it burns, and sometimes brilliantly. When heated, it sublimes in a crystalline form, and those crystals obtained from its solution in ether are quadrangular plates. It appears to be composed of three atoms of chlorine = 100.5, and two atoms of carbon = 11.4.

It is soluble in oils, both volatile and fixed. At a red-heat, but not below it, oxygen acts upon it; the electric spark did not inflame a mixture of the vapour of it with oxygen even at about 400° Fahr. nor did hydrogen cause any change in it under the same circumstances; but on passing them through a red-hot tube, decomposition took place. Muriatic acid gas was formed, and carbon deposited. Sulphuric acid does not appear to act upon it at all. The metals for the most part decompose the perchloride of carbon at high temperatures. Potassium burns brilliantly in the vapour of it; and iron, tin, &c. combine with the chlorine, and deposit the carbon.

When passed through a red-hot tube, it is decomposed, chlorine is evolved, and the protochloride of carbon obtained.

Protochloride of Carbon.—This is formed by heating the perchloride to dull redness; it at first sublimes, but eventually chlorine is evolved, and the protochloride formed; and being vaporized is condensed into a fluid. By repeated distillations it is procured perfectly colourless and limpid, and possessing the following properties. Its specific gravity 1.5526. It is a non-conductor of electricity; its refractive power is very nearly that

of camphor. It is incombustible, except in the flame of a spirit-lamp, and it then yields muriatic acid, with a bright yellow flame.

It remains fluid at 0° of Fahr.; heated under water to about 165° , it is vaporized; and while the temperature is continued, remains so. When strongly heated by being passed over rock crystal in a glass tube, a portion of it is decomposed; and carbon is deposited in the tube; but whether this decomposition is to be attributed merely to the heat, or to action upon the glass, is uncertain. Like the perchloride, it does not unite with water, but it combines with alcohol and ether; the solutions burn with a greenish flame, and muriatic acid fumes are perceptibly evolved. The fixed and volatile oils also combine with it. The metals act upon it nearly in the same way as upon the perchlorides; potassium acts slowly upon it at common temperatures, but when heated in the vapour of the protochloride, brilliant combustion ensues, and carbon is deposited. Neither nitric, muriatic, nor sulphuric acid, act upon it, and it does not combine with alkaline solutions. Hydrogen passed through the vapour of this chloride decomposes it at a high temperature, carbon is deposited, and muriatic acid formed. It appears to consist of one atom of each of its constituents, or 33.5 chlorine + 5.7 carbon.

Mr. Faraday has also in this paper given an account of a triple compound of iodine, carbon, and hydrogen. It was procured by exposing iodine in olefiant gas to the solar rays. Crystals were gradually formed; no hydriodic acid appeared to exist in the vessel; and hence olefiant gas had not been decomposed, but merely absorbed by the iodine. The triple compound of iodine, carbon, and hydrogen, was purified by potash, which dissolved the uncombined iodine. It is a colourless, crystalline, friable body; its taste is sweetish, and its smell aromatic. It is a non-conductor of electricity. By heat, it is first melted, and then sublimes unchanged, and condenses into crystals which are either tubular or prismatic. At a high temperature it suffers decomposition with the evolution of iodine. It burns in the flame of a spirit-lamp, giving iodine and hydriodic acid. It is soluble in ether and alcohol, but not in water, acid, or alkaline solutions. At between 300° and 400° , it is decomposed in sulphuric acid, but probably by the mere heat. Mr. Faraday considers this substance as analogous to chloric ether. He proposes to call it hydrocarburet of iodine. Mr. Faraday has not yet procured an iodide of carbon; but he entertains well-grounded hopes of being able to succeed when the sun's rays are more powerful than at this season of the year.

On account of the extreme importance of the facts contained in the paper of which I have now given an outline, I shall take the earliest opportunity permitted by the Royal Society of laying the whole of it before the reader.

GEOLOGICAL SOCIETY.

The meetings of this Society were resumed on Nov. 3, when the following papers were read :

A notice accompanying specimens of Lead Ore in Toadstone, from near Matlock, Derbyshire, by Charles Stokes, Esq. MGS. was read.

It has been a prevalent opinion that the veins of lead ore which occur in the limestone of Derbyshire do not traverse the beds of toadstone which alternate with the limestone. It appears, however, from the information of Mr. Tissington, that this is an error. A vein had been worked in the limestone above the toadstone at the Seven Rakes mine, and abandoned on coming to toadstone from the general idea that the veins were cut off by it. About a year ago, however, some miners ascertained that the vein continues through the toadstone, and it is now working with profit. Another vein in the side mine is also working in the toadstone.

A notice of fossil plants having been found at the Col de Balme, by H. T. de la Beche, Esq. MGS. was read.

The Col de Balme at the head of the valley of Chamouny, which by M. Ebel is stated to consist of primitive rocks, is composed of beds of clay-slate, of limestone, and of a few thin beds of sandstone : these seem to be a continuation of the limestones which are remarked in patches in the valley of Chamouny, and which probably once occupied the whole length of the valley.

While crossing the Col de Balme in the autumn of 1819, M. de la Beche collected specimens of the thin beds of sandstone containing vegetable impressions precisely similar to those which are usually found in coal formations. He also procured at Chamouny some specimens of clay slate found on the Col de Balme, and which had impressions of the plants of coal formations covered by beautiful white talc in very thin laminæ.

The "Supplementary Remarks on Quartz Rock," by Dr. Macculloch, MGS. were read.

A further examination of the westward of Sunderland, where gneiss abounds in all its varieties from the granite to the schistose, has enabled Dr. Macculloch to ascertain its alternations with quartz rock. The latter appears sometimes in a detached state, occupying the summits of those mountains of which the chief mass consists of gneiss, as is the case in Ben Stack, and in the northernmost Ben More; or it is found covering extensive tracts of country, and occupying the lowest as well as the highest land, as in the instance of the great tract from Ben More Assynt to Canasp and Curiach.

On the north side of Glendhu, the bed of quartz rock is about 100 feet thick: it reposes on the gneiss which extends to the western sea, and consists of very even and parallel strata, of an extremely compact variety. This mass is followed by a similar

gneiss to that which lies below it, and in crossing the ridge to Lochmore, the quartz is again found on one of the inferior summits.

In Ben Ay, Ben Klibrigg, and other parts of Sutherland, there appears a gradual transition between the two rocks; the gneiss first assuming the aspect of micaceous schist, which then passes into a micaceous and highly schistose quartz rock, and ultimately into pure quartz.

In many parts of the coast of Sutherland, quartz rock is found alternating with red sandstone, both in single beds and in mountain masses. The same appearances are observable in several parts of Ross-shire. The quartz rock of which the entrance of the eastern side of Loch Eribol is composed, and which is remarkable for the magnitude and number of the caves which it forms, contain pyrites in considerable quantity; and in consequence of the decomposition of the substance, is frequently stained of a reddish brown colour so as to resemble granite. In this neighbourhood the quartz rock presents examples of considerable curvatures.

In the district of Mar, Dr. Macculloch has found a perpetual alternation of gneiss and micaceous schist with quartz rock, and the apparent transition between the two is so perfect that without a minute examination of the fractured rock, it is impossible to determine which it presents. In the northern parts of Aberdeenshire, this association is less common.

The last conjunction of quartz rock observed by Dr. Macculloch consists of a repeated alternation with micaceous schist, chlorite schist, and hornblende schist. This series extends over a considerable tract of country in a regularly stratified manner, the beds of the different rocks rarely exceeding a few yards, and often being only a few inches in thickness.

Quartz rock has not been hitherto found to contain any imbedded minerals, except pyrites and garnets.

Nov. 17.—The "Description of a Visit to the Nilghurries, a Range of Hills in the Coimbatore District," (East Indies) by Nathaniel Kindersley, of the Madras Civil Service; communicated by B. Babington, Esq. was read.

Dec. 1.—A letter from G. Mantell, Esq. accompanying the drawing of a fossil vegetable from the Sussex sandstone, was read.

This specimen, which is from the beds of sandstone that occur in the Oak-tree Clay of the south-eastern part of Sussex, derives additional interest from the circumstance of its being associated with the remains of the *Ichthyosaurus*, and of some species of *Lacerta*. It bears a closer affinity to the *Euphorbia* than to any other recent vegetable; and it strikingly resembles the fossil plants of the gritstone of Derbyshire and other places.

A paper "On the Valley of the Sutleig River in the Himalaya Mountains," by H. T. Colebrooke, Esq. VPGS. was read.

In a journey undertaken in the autumn of 1818 by Lieut. A. Gerard, accompanied by his brother, for the purpose of approximating towards the extreme height of Mánabarover by barometrical measurement of the bed of the Suttleig, a variety of specimens illustrative of the geological structure of the lower and middle valleys were collected, but in consequence of the many difficulties which were encountered on the route, and the accidents which happened to the travellers during their progress, this series has been rendered less perfect than it otherwise would have been.

The banks of the Suttleig, in the lower valley, at the elevation of 2000 feet above the level of the sea, are composed of limestone which is apparently primitive. The general inclination of the strata is stated to be 10° or 15° , and the direction much diversified. At Jauré, on the northern bank, hot springs issue within two or three feet from the river. A thermometer plunged into one of them rose to 130° of Fahrenheit; while the temperature of the river was 61° . The water has a strong sulphureous smell, and encrusts the pebbles among which it runs with a yellow substance. Limestone seems the prevailing rock on the hills which bound the adjacent valleys. Among the specimens is a stalactite from the roof of a cave near the top of the Carol mountain, and about 6500 feet above the level of the sea.

In crossing the Himalaya at the Bruavy pass, which is the route of communication between the middle valley of the Suttleig and the valley of the Paber, and of which the extreme altitude is 15,000 feet, mica slate, gneiss, and granite (some of the specimens containing garnets, and others tourmaline), were found; and veins of quartz and mica, and quartz and hornblende, are observable in the specimens which have been transmitted.

The mean height of the Suttleig near to its influence with the Bespa is 6,300 feet. The rocks which here form its banks are inclined 25° — 30° , and dip eastward: they consist of granite, gneiss, quartz rock, granular quartz; quartz, and mica, and granite with hornblende. Between this spot and Rispé, from 6500 to 9800 feet above the level of the sea, the rocks are chiefly formed of a whitish crumbling granite. The Cailas, or Baldang mountains on the south, an assemblage of pointed peaks, covered with snow, and more than 20,000 feet in height, are to all appearance composed of the same kind of rock.

Overhanging the town of Marangis is a mountain of clay slate. Upon it, at an elevation of 12,000 feet, heath, juniper, and gooseberry bushes, are growing. In advancing to the Tunzang pass, which is 13,740 feet above the sea, rocks formed chiefly of compact quartz, with chlorite, were observed. The pass itself exhibited clay slate, with pyrites and globular mica.

A few miles further on, granite, gneiss, mica slate, quartz and mica, actinolite, with quartz and garnets, pyrites in quartz, a bluish-grey limestone, with white veins and calcareous tufa, were

found. Here the strata, according to Lieut. Gerard's observation, run north-west to south-east, and dip to the north-east, at an angle of 40° or 45° .

In the neighbourhood of Namptusangi, the bed of the Sutleig is 8220 feet above the sea, and consists of only two sorts of rock, viz. mica slate and granular quartz, with imperfectly crystallized hornblende. At its confluence with the Lé river, the banks are composed of granite. On ascending the latter stream, the banks were found to exhibit specimens of slate, potter's clay, marl, or loam and sand, and stalactitic carbonate of lime. Higher up the same river, and in the vicinity of Chango, where the bed is not less than 9900 feet above the sea, primitive limestone, blue and likewise greyish-white, with disintegrated pyrites, were found; also mica slate, with fragments of veins. In one instance, white quartz with mica, hornblende, and garnet; in another, actinolite, with quartz, mica, and garnet.

Between Namgia and Shisské, where the survey eastward terminated, and where the bed of the river is 9000 feet above the level of the sea, the rocks are composed of granite, with and without tourmaline and garnets, gneiss, mica slate, compact quartz, cyanite, with quartz and mica, and compact felspar, with hornblende.

North of the pass of the Shisské-ghuté is situated the Tarhigang mountains, which Lieut. Gerard ascended to the prodigious height of 19,411 feet above the level of the sea, and within two miles of the top, which is estimated at 22,000 feet.

The rocks here lie in immense detached masses, heaped upon one another. One specimen of whitish primitive limestone, and another of granite, with tourmaline and garnet, which were found at this station, have been preserved.

The Rol or Shatul pass, over the Himalay mountains, by which the surveyors returned from this arduous journey, is nearly 15,000 feet high. The rocks at the summit of this pass consist of gneiss; and the peaks on each side, rising to an elevation of nearly 3000 feet more, appear to be formed of the same materials. On descending the southern side from the Rol, at the height of 12,000 feet above the sea, the rocks are found generally composed of gneiss; and on the northern side, the prevalent rock was ascertained to be granular quartz.

Seeds of a species of *Campanula* were gathered at the elevation of 16,800 feet above the level of the sea, at a spot where the thermometer at noon in the middle of October was 27° of Fahr. Shrubs were found in a vegetating state at a still greater altitude.

Dec. 15.—“A Notice respecting the Cally Copper-Mine in Scotland,” by John Taylor, Esq. Treasurer of the Geolog. Soc. was read. This mine is situated near Gatehouse, a small town in Kircudbrightshire, on the mail road from Dumfries to Port Patrick. Veins producing lead had been worked there for

several years, but the existence of copper was not suspected until some labourers who were employed to drain a swampy piece of ground near the top of the hill of Cally, discovered several masses of a rich ore. The mine has been since worked under the direction of a company, and has already produced 60 to 70 tons of copper, worth nearly 15*l.* per ton. The direction of the veins which run east and west of the shaft, that was sunk to the depth of eight fathoms, on the spot where the ore was first found, and seem to incline together, holds forth the prospect of a regular vein in depth.

The ores are of a mixed character, containing the yellow and coated varieties, with some green carbonate.

A paper "On a recent deposit of Compact Limestone," by Charles Stokes, Esq. MGS. was read.

On examining a congeries of fragments of recent corals and shells, supposed to be from the Mediterranean, portions of a compact limestone were found enveloping and encrusting the corals and shells; and as it is well known that corals cannot penetrate substances that are much softer than compact limestone, it necessarily follows that this limestone must be a recent deposit. One of the corals, discovered by a fortunate fracture, retains all its beautiful structure, although completely imbedded in limestone.

An extract of a letter from Mr. William Jack to H. T. Colebrooke, Esq. VPGS. dated Feb 5, 1820, and communicated by Mr. Colebrooke, was read.

The island of Penang off the Malay Peninsula is entirely composed of granite, varying in the fineness and coarseness of its grain, and sometimes in containing hornblende. In the island of Singapore, the rocks are secondary, the principal one is a red sandstone, of which the strata have a considerable dip to the south, or south-eastward, and which changes in some parts to a breccia or conglomerate. A small hill near the town is composed of argillaceous ironstone. These are the prevailing minerals along the eastern coast of the Peninsula; and the last mentioned strata appear to be connected with the alluvial deposits of tin, which are so abundant on this coast. The principal mines are at Salengore and Pera, where the tin is found in horizontal beds alternating with clay strata, and so pure as only to require to be washed and reduced. Near this tin, the primitive and volcanic ranges seem to meet; the former descending in a south-eastern direction through the Malay Peninsula, and the parallel island of Sumatra, from the Himalaya range, until it comes in contact with the volcanic series, which runs from thence nearly east, through Java and the chain of islands that lie off its eastern extremity.

ARTICLE XV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS
CONNECTED WITH SCIENCE.

I. Test for Barytes and Strontia.

Barytes and strontia may be readily distinguished from each other by the following process: Make a solution of the earth, whichever it may be, either by nitric, muriatic, or some other acid, which will form a soluble salt with it: add solution of sulphate of soda in excess, filter, and then test the clear fluid by subcarbonate of potash. If any precipitate falls down, the earth was strontia; if the fluid remains clear, it was barytes.—(Journal of Science and the Arts, vol. x. p. 189.)

II. Ammoniacal Alum.

M. Anatole Riffault has detailed a set of experiments made upon ammoniacal alum. No analysis of this salt, he states, is to be found in any treatise on chemistry. He observes also that a knowledge of its composition is necessary to make an exact analysis of the different varieties of alum, some of which contain a quantity of this salt, especially those which are not manufactured in France. Having purified the ammoniacal alum of commerce by repeated crystallizations, so as to free it from iron, it was dried by exposure to the air; 13 gr. 420 of this salt were first gently, and afterwards strongly, heated in a platina crucible for an hour and a half; the residuum was pure alumina; it weighed 1 gr. 603, equal to 11.944 per cent. On repeating the experiment, the alumina amounted to 11.869 per cent. giving a mean of 11.906 per cent. of alumina in ammoniacal alum.

To determine the quantity of sulphuric acid, a solution of 6 gr. 522 of the alum was decomposed by muriate of barytes; the sulphate weighed 7 gr. 010, = 2 gr. 3547 of sulphuric acid, or 36.104 per cent. This experiment on repetition gave 35.991 per cent. or a mean of 36.042.

M. Riffault concludes, that in the ammoniacal alum 11.906 of alumina are combined with 26.979 of sulphuric acid, the remaining 9.063 of the acid uniting with 3.898 of ammonia. The composition of ammoniacal alum he, therefore, states to be:

Sulphate of ammonia.....	12.961
— alumina.....	38.885
Water.....	48.154
	<hr/>
	100.000

The atomic constitution of this salt he considers as

One atom of sulphate of ammonia.....	71.67	12.838
Three atoms of sulphate of alumina.....	216.69	38.816
Twenty-four atoms of water.....	269.88	48.346
	<hr/>		<hr/>
	558.24		100.000

In the next number of the *Annals* I shall probably state the results of some experiments which I have made on alum, including the ammoniacal alum above mentioned.

III. Sulphate of Indigo as a Test to determine the Strength of Solutions of Oxymuriate of Lime.

It is well-known that bleachers who employ solution of oxymuriate (chloride) of lime, are in the practice of determining its strength by the quantity of a given

solution of sulphate of indigo which it is capable of discharging. From some experiments made on this subject, I find that this test is not by any means to be relied upon, as will appear by the following statement:

I prepared solutions of oxymuriate of lime and sulphate of indigo of such a degree of strength that equal measures, when mixed, destroyed the colour of the indigo. I then diluted the solution of oxymuriate of lime in the following proportions, and with the annexed results:

One measure of solution of oxymuriate of lime,

One measure of water. This mixture destroyed the colour of $\frac{1}{10}$ th measure of solution of indigo.

One measure of solution of oxymuriate of lime,

Three measures of water; destroyed the colour of $\frac{1}{4}$ th measure of solution of indigo.

One measure of solution of oxymuriate of lime,

Seven measures of water. This discharged the colour of $\frac{1}{70}$ th measure of solution of indigo.

It appears, therefore, that by mere dilution, the discharging power of oxymuriate of lime is much increased; an equal quantity of water raising its power one-fifth, three times its bulk, one-fourth; and seven times its quantity, four-tenths.

Dr. Henry, to whom I mentioned these facts, accounted for them (if I remember rightly) by supposing that when the concentrated solution of oxymuriate of lime is used, the sulphuric acid of the sulphate of indigo expels a portion of chlorine in the state of gas, and which has no action upon the colouring matter. I think it is worth the consideration of bleachers, whether some coloured solution could not be adopted free from this ambiguity; for it must be evident that the bleaching power of the oxymuriate is not accurately expressed by that of its discharging the colour of sulphate of indigo. A standard solution of cochineal might be made and preserved by common salt; at least, I have no doubt it may be so kept, for I find that blue vegetable infusions used as tests for acids and alkalies are very perfectly preserved by it.

IV. On the Application of Chromate of Lead, to Silk, Woollen, Linen, and Cotton. By M. J. L. Lassaigne.

The colouring matters which were formerly fixed upon cloths were derived from organized matter; none were supplied by the mineral kingdom, although so rich in colouring combinations unalterable by the air; and it is only within a few years that some of these mineral productions have been applied to the art of dyeing.

M. Raymond, of Lyons, was the first, who, by a process, equally simple and ingenious, fixed prussian blue upon silk, and gave to the art of dyeing a brilliant and unchangeable colour.

Last year, M. Braconnot, of Nancy, by applying sulphuret of arsenic (orpiment) to cloths, procured a yellow colour, but less durable than the former.

In the course of some experiments upon chromate of lead, I succeeded in combining this salt with all cloths, by a process similar to that which M. Raymond employed to dye silk with prussiate of iron.

After having immersed well-cleaned skeins of silk for a quarter of an hour in a weak solution of subacetate of lead, I took them out, and washed them in a large quantity of water; the object of this preparation was to combine the silk with a certain quantity of subacetate of lead. These cloths, thus prepared, were afterwards immersed in a weak solution of neutral chromate of potash,* they took a fine yellow colour, which continued to increase; at the end of ten minutes, the effect ceased, that is to say, the skeins of silk had received the maximum of colour, which could be yielded by the quantity of subacetate of lead with which they were combined; they were then washed and dried.

*The solution of native chromate of iron, decomposed by nitrate of potash, and saturated with nitric acid, may be employed with equal advantage.

This colour, like those supplied by the mineral kingdom, was unalterable by exposure to the air; by varying the proportions of sub-acetate of lead, and chromate of potash, tints varying from pale to deep yellow may be obtained.

The same process is applicable to woollen, cotton, and linen; but it is better to digest these different cloths in a solution of acetate of lead, heated to about 180° of Fahrenheit.

From the inconvenience which this colour possesses as well as the former, of being partially decomposed by soap, I am of opinion that it can be employed only for dyeing silk.—(Ann. de Chim. vol. xv. p. 77.)

V. Process for procuring pure Zirconia.

Powder the zircons very fine, mix them with two parts of pure potash, and heat them red hot in a silver crucible for an hour. Treat the substance obtained with distilled water, pour it on a filter, and wash the insoluble part well; it will be a compound of zirconia, silice, potash, and oxide of iron. Dissolve it in muriatic acid, and evaporate to dryness, to separate the silice. Redissolve the muriates of zirconia and iron in water; and to separate the zirconia which adheres to the silice, wash it with weak muriatic acid, and add it to the solution. Filter the fluid, and precipitate the zirconia and iron by pure ammonia; wash the precipitates well, and then treat the hydrates with oxalic acid, boiling them well together, that the acid may act on the iron, retaining it in solution whilst an insoluble oxalate of zirconia is formed. It is then to be filtered, and the oxalate washed, until no iron can be detected in the water that passes. The earthy oxalate is, when dry, of an opaline colour; after being well washed, it is to be decomposed by heat in a platinum crucible.

Thus obtained, the zirconia is perfectly pure, but is not affected by acids. It must be re-acted on by potash as before, and then washed until the alkali is removed. Afterwards dissolve it in muriatic acid, and precipitate by ammonia. The hydrate thrown down, when well washed, is perfectly pure, and easily soluble in acids.—(MM. Dubois and Silveira, Ann. de Chim. vol. xiv. p. 110.)

ARTICLE XVI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Shortly will be published A Series of Questions and Answers in the Practice of Physic, Materia Medica, &c. written for the Use of Gentlemen preparing for their Examination at Apothecaries' Hall. By C. M. Syder, Surgeon.

Mr Arrowsmith has nearly ready for publication, A Map of the Constellations, in two large sheets, accompanied by a memoir.

Shortly will be published, The Principles of Foreign Medicine, explained, illustrated, and applied to British Practice. By J. G. Smith, MD.

Dr. Ramsbottom has nearly ready, Practical Observations on Midwifery, with a Selection of Cases.

Dr. Ayre is printing a new edition, with additions, of his Practical Observations on Disorders of the Liver.

JUST PUBLISHED.

A System of Chemistry, in Four Volumes, 8vo. By Thomas Thomson, MD. Regius Professor of Chemistry in the University of Glasgow, &c. &c. Sixth Edition, revised and corrected throughout. Price 3*l.* boards.

A Dissertation on the Treatment of Morbid Local Affections of the Nerves. By Joseph Swan. 8vo. 10*s.* 6*d.*

An Essay on the Diagnosis between Erysipelas, Phlegmon, and Erythema. By George Hunne Weatherhead, MD. 8vo. 4*s.*

The Pharmacopœia of the Royal College of Physicians of London, 1809, literally translated by George Fred. Collier, Surgeon. 8vo. 10s. 6d.

Elements of Chemistry, with its Application to explain the Phenomena of Nature, &c. By James Millar, MD. 8vo. 12s.

Sound Mind, or Contributions to the Natural History and Physiology of the Human Intellect. By J. Haslam. 8vo. 7s.

Practical Treatise on the Diseases of the Eye. By John Vetch, MD. 8vo 10s. 6d.

A Treatise on Mildew, and the Cultivation of Wheat, including many Agricultural Hints. By Francis Blaikie. 1s. 6d.

The Botanical Cultivator, or Instructions for the Management of Plants cultivated in the Hot-Houses of Great Britain. By Robert Sweet, FLS. 8vo. 10s. 6d.

Grisenthwaite's New Theory of Agriculture, in which the Nature of Soils, &c. is explained. By J. C. Curwen, Esq. MP. 8vo. 5s.

View of the Intellectual Powers of Man, with Observations on their Cultivation, adapted to the present State of this Country. 8vo. 5s.

ARTICLE XVII.

NEW PATENTS.

William Acraman, Jun. and Daniel Wade Acraman, of Bristol, for improvements in the processes of forming the materials for manufacturing chains and chain-cables. Oct. 16, 1820.

Joseph Main, Esq. of Bagno-court, Newgate-street, London, for improvements on wheeled-carriages. Oct. 20.

James Richard Gilmour, of King-street, Southwark, and John Bold, of Millpond Bridge, Surrey, for improvements on printing-presses. Oct. 20.

Thomas Prest, of Chigwell, Essex, for a new and additional movement to a watch to enable it to be wound up by the pendant knob, without any detached key or winder. Oct. 20.

John Bickinshaw, of Bedlington Iron Works, in the county of Durham, for improvements in manufacturing and construction of a wrought or malleable iron rail road or way. Oct. 23.

William Taylor, of Wednesbury, Staffordshire, furnace-worker, for an improved furnace for smelting iron and other ores. Oct. 23.

Thompson Pearson, of South Shields, for an improvement on rudders. Nov. 1.

Henry Lewis Lobeck, of Tower-street, London, for an improvement in the process of making yeast. (Communicated by a foreigner to him.) Nov. 1.

Samuel Wellman Wright, of Upper Kensington, Surrey, for a combination in machinery for making bricks and tiles. Nov. 1.

Peter Hawker, of Long Parish House, near Andover, for a machine, instrument, or apparatus, to assist in the proper performance on the piano-forte, or other keyed instruments. Nov. 1.

Thomas Bonsor Crompton, of Farmworth, Lancaster, for an improvement in drying and finishing paper, by means hitherto unused for that purpose. Nov. 1.

William Swift Torey, of Lincoln, for certain improvements in drills, to be affixed to ploughs. Nov. 1.

John Winter, Esq. of Acton, Middlesex, for improvements on chimney-caps, and in the application thereof. Nov. 7.

William Carter, of St. Agnes Circus, Old-street road, printer, for improvements in steam-engines. Nov. 11.

Thomas Dyson, of Abbey Dale, Sheffield, for an improvement, or improvements of plane irons and turning chisels. Nov. 11.

ARTICLE XVIII.

Astronomical, Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*Latitude $51^{\circ} 37' 44.3''$ North. Longitude West in time $1^{\circ} 20' 93''$.*Astronomical Observations.*

Nov. 1. Emission of Jupiter's first { $7^h 40' 51''$ Mean Time at Bushey.
satellite. { $7^h 42' 12''$ Mean Time at Greenwich.

Magnetical Observations, 1820. — Variation West.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Nov. 1	8h 35'	24° 31'	46"	1h 25'	24° 38'	10"	Owing to the shortness of the days, evening observations discontinued.	
2	8 40	24 31	36	1 30	24 37	58		
3	8 40	24 32	12	1 35	24 37	48		
4	8 35	24 32	18	1 20	24 38	09		
5	8 45	24 31	37	1 20	24 37	16		
6	8 40	24 32	21	1 20	24 38	00		
7	—	—	—	1 25	24 40	59		
8	8 40	24 32	32	1 30	24 38	08		
9	8 40	24 32	27	1 20	24 38	00		
10	8 40	24 32	08	1 35	24 37	28		
11	8 35	24 32	10	1 25	24 37	35		
12	8 40	24 32	50	1 15	24 37	28		
13	8 40	24 31	45	—	—	—		
14	8 35	24 39	50	1 25	24 47	16		
15	8 35	24 37	52	1 20	24 36	54		
16	8 35	24 32	25	1 20	24 36	56		
17	8 40	24 31	23	—	—	—		
18	8 40	24 31	58	1 25	24 37	33		
19	8 45	24 31	30	1 25	24 37	18		
20	8 40	24 31	50	1 25	24 47	21		
21	8 40	24 34	57	1 25	24 47	45		
22	8 35	24 32	06	1 55	24 36	39		
23	8 35	24 31	38	1 25	24 37	25		
24	8 40	24 32	49	1 25	24 37	19		
25	8 45	24 33	08	1 20	24 37	46		
26	8 35	24 33	00	1 20	24 37	02		
27	8 45	24 33	39	1 35	24 37	20		
28	8 35	24 32	44	1 15	24 36	46		
29	8 40	24 33	22	1 10	24 36	58		
30	—	—	—	—	—	—		
Mean for the Month.	8 39	24 32 23		1 25	24 37 38			

In taking the mean of the observations, those on the 14th and those of the morning of the 15th are rejected, being so great; and it is remarkable that this excess continued only on the falling of snow. On the weather clearing up prior to observing the noon variation of the 15th, it had decreased. The noon observation of the 20th is likewise rejected: this increase was attended with drizzling rain.

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Nov.		Inches.				Feet.		
1	Morn...	28.893	43°	76°	WNW		Rain	42°
	Noon...	28.977	43	73	NW by W		Showery	44½
	Even...	—	—	—	—		—	—
2	Morn...	29.274	33	74	WSW		Very fine	33
	Noon...	29.283	45	65	W by S		Very fine	46
	Even...	—	—	—	—		—	—
3	Morn...	29.400	37	72	NW		Clear	35
	Noon...	29.400	45	60	W		Very fine	46½
	Even...	—	—	—	—		—	—
4	Morn...	29.392	37	72	ENE		Cloudy	34½
	Noon...	29.399	45	68	ENE		Fine	46½
	Even...	—	—	—	—		—	—
5	Morn...	29.455	36	63	SSW		Fine	32
	Noon...	29.400	38	71	SE by S		Cloudy	49½
	Even...	—	—	—	—		—	—
6	Morn...	29.267	48	87	WSW		Misty	38
	Noon...	29.294	50	73	WSW		Cloudy	50
	Even...	—	—	—	—		—	—
7	Morn...	29.284	—	90	SE by S		Fog	46
	Noon...	29.295	54	70	SSW		Cloudy	54
	Even...	—	—	—	—		—	—
8	Morn...	29.375	52	84	E by S		Fog	51
	Noon...	29.369	54	77	ENE		Rain	54
	Even...	—	—	—	—		—	—
9	Morn...	29.469	45	74	ENE		Fine	44
	Noon...	29.467	46	64	ENE		Cloudy	47½
	Even...	—	—	—	—		—	—
10	Morn...	29.543	42	74	NE		Sm. rain	41
	Noon...	29.547	45	60	NE		Cloudy	46½
	Even...	—	—	—	—		—	—
11	Morn...	29.678	41	74	N by E		Cloudy	40
	Noon...	29.718	47	62	NE		Fine	47½
	Even...	—	—	—	—		—	—
12	Morn...	29.648	38	69	W by N		Fine	32½
	Noon...	29.500	40	68	W by S		Sm. rain.	41
	Even...	—	—	—	—		—	—
13	Morn...	29.094	36	77	NNE		Sm. rain.	35½
	Noon...	29.048	—	78	NNE		Sleet	36½
	Even...	—	—	—	—		—	—
14	Morn...	29.800	34	75	NNE		Snow	34½
	Noon...	29.294	35	70	NE		Ss. show.	27
	Even...	—	—	—	—		—	—
15	Morn...	29.433	32	71	NNE		Very fine	31½
	Noon...	29.439	40	63	NE		Fine	41
	Even...	—	—	—	—		—	—
16	Morn...	29.464	33	75	N		Clear	32
	Noon...	29.400	39	61	NNE		Cloudy	39½
	Even...	—	—	—	—		—	—
17	Morn...	29.163	32	73	SE		Snow	28½
	Noon...	29.103	—	78	SSE		Rain	40½
	Even...	—	—	—	—		—	—
18	Morn...	29.339	36	77	NE by E		Very fine	34
	Noon...	29.379	42	68	N by W		Very fine	41½
	Even...	—	—	—	—		—	—

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six'a.
		Inches.				Feet.		
Nov.	Morn....	29.432	37°	80°	SE		Misty	34
19	Noon....	29.450	44	72	SSW		Misty	44
	Even....	—	—	—	—		—	40½
	Morn....	29.500	43	84	S by W		Sm. rain	47
20	Noon....	29.464	47	76	SSW		Sm. rain	46½
	Even....	—	—	—	—		—	50½
	Morn....	29.408	47	80	S by W		Cloudy	45
21	Noon....	29.400	50	67	S		Fine	47½
	Even....	—	—	—	—		—	41½
	Morn....	29.308	45	75	SE by S		Rain	46
22	Noon....	29.263	45	76	ESE		Fog, rain	36
	Even....	—	—	—	—		—	43½
	Morn....	29.085	42	84	E by S		Fog	40½
23	Noon....	29.982	46	83	N by W		Rain	42½
	Even....	—	—	—	—		—	47
	Morn....	29.208	38	72	SSE		Cloudy	40½
24	Noon....	28.217	43	77	ESE		Fog, rain	42½
	Even....	—	—	—	—		—	47
	Morn....	29.090	42	82	ESE		Cloudy	40½
25	Noon....	29.083	45	75	ESE		Fine	42½
	Even....	—	—	—	—		—	50½
	Morn....	29.319	44	83	SSE		Cloudy	40½
26	Noon....	29.390	49	70	SSW		Fine	47½
	Even....	—	—	—	—		—	36
	Morn....	29.438	41	78	E		Fine	38
27	Noon....	29.438	47	64	E		Fine	36
	Even....	—	—	—	—		—	39½
	Morn....	29.603	38	82	ENE		Cloudy	39
28	Noon....	29.628	37	78	NE		Cloudy	39½
	Even....	—	—	—	—		—	39
	Morn....	29.800	87	73	E by N		Cloudy	39½
29	Noon....	29.800	38	70	NE		Cloudy	39
	Even....	—	—	—	—		—	39½
	Morn....	29.773	—	79	NE by N		Cloudy	39½
30	Noon....	—	—	—	—		—	39½
	Even....	—	—	—	—		—	39½

Rain, by the pluviometer, between noon the 1st of Nov. and noon the 1st of Dec. 1.223 inch. The quantity that fell upon the roof of my observatory during the same period, 1.303 in. Evaporation, between noon the 1st of Nov. and noon the 1st of Dec. 0.853 in.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a.m.
		Max.	Min.	Max.	Min.			
11th Mo.								
Nov. 1	N W	29.98	29.59	56	34	—	06	78
2	W	30.07	29.98	48	25	—	—	72
3	Var.	30.10	30.07	41	23	—	—	91
4	N	30.15	30.09	48	27	—	—	91
5	S W	30.15	29.96	50	30	—	20	91
6	W	29.96	29.91	52	44	—	—	87
7	S W	30.05	29.91	57	50	—	02	93
8	E	30.14	30.05	56	45	—	—	93
9	E	30.20	30.14	50	41	—	—	70
10	N	30.37	30.20	46	39	—	—	71
11	N E	30.37	30.33	49	34	—	—	76
12	S W	30.23	29.77	43	36	40	24	66
13	N	29.99	29.77	49	31	—	13	81
14	N E	30.15	29.99	38	31	—	02	62
15	N E	29.99	29.94	43	29	—	—	76
16	N W	29.94	29.72	41	23	—	—	84
17	S E	29.84	29.63	43	29	—	30	78
18	Calm	29.94	29.84	40	29	—	01	92
19	S E	29.97	29.94	47	33	—	—	90
20	S E	29.94	29.90	50	45	—	—	90
21	S E	29.90	29.82	53	45	—	—	84
22	S E	29.82	29.60	49	39	—	40	86
23	Var.	29.71	29.45	48	30	—	26	90
24	S W	29.62	29.45	46	32	—	—	91
25	E	29.81	29.62	48	43	—	16	94
26	S E	29.92	29.81	52	38	—	—	80
27	E	30.08	29.92	46	33	—	—	92
28	E	30.26	30.08	39	36	—	—	83
29	N E	30.26	30.23	41	33	—	—	70
30	N	30.23	30.17	43	29	55	02	71
		30.37	29.45	57	23	95	1.82	94—62

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eleventh Month.—1. Rainy: a fine arch of *Cirrocumulus* stretching from NW to SE, and coloured a bright red by the setting sun. 2. Day very fine: night foggy. 3. A very thick fog in the morning. 4, 5. Hoar-frost: foggy. 6. Fine. 7. Cloudy. 8. Cloudy. 9, 10, 11. Fine. 12, 13. Rainy. 14. Cloudy: windy: a little snow about noon. 15. Cloudy: some hail. 16. White-frost: fine. 17. Some snow in the morning. 18. An extremely thick fog, which remained most of the morning. About ten o'clock the coachmen on the road were unable to see the heads of their horses, which, in many instances, were obliged to be led. 19. Fine. 20. Cloudy: fine. 21. Very fine. 22. Rainy. 23. Morning very rainy: a rainbow about half-past one, p.m. 24. Cloudy. 25. Fine. 26. Fine. 27, 28, 29, 30. Cloudy.

RESULTS.

Winds: N, 4; NE, 4; E, 5; SE, 6; SW, 4; W, 2; NW, 2; Calm, 1; Var. 2.

Barometer: Mean height

For the month..... 29.968 inches.

For the lunar period, ending the 26th..... 29.864

For 13 days, ending the 1st (moon north)..... 29.189

For 14 days, ending the 15th (moon south)..... 30.376

For 14 days, ending the 29th (moon north)..... 29.857

Thermometer: Mean height

For the month..... 49.8°

For the lunar period, ending the 26th..... 41.6

For 30 days, the sun in Scapio..... 42.883

Evaporation..... 0.95 in.

Rain..... 1.82

Mean of hygrometer..... 82°

Laboratory, Stratford, Twelfth Month, 18, 1820.

R. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1821.

ARTICLE I.

Researches into the Mathematical Principles of Chemical Philosophy. By J. B. Emmett.

(Continued from vol. xvi. p. 358, *First Series*.)

Hull, Dec. 28, 1820.

Radiation of Caloric.

WHEN a mass of matter is placed in a heated medium, it, for a definite length of time, is receiving heat; i. e. each particle, as well as the entire mass, by prop. 6, cor. and prop. 11, by attracting the surrounding caloric, becomes encompassed by a calorific atmosphere, which is of uniform density in each part of the concentric strata, and which is more dense than the atmosphere which is produced by exposure to a medium of lower temperature; during this time, the surrounding medium experiences a diminution of temperature in those parts which are in the vicinity of the cold mass, which diminution of heat continually decreases, and the body arrives at the precise temperature of the surrounding medium, which then becomes uniform in all its parts. The reason why the temperature of the body and of the medium are uniform, although the caloric is more dense in those parts which are near the mass of matter than in the more remote, is evident; for by temperature is to be understood the tendency which caloric has to escape, or its sensible elastic force: now the increased elastic force of a calorific atmosphere, arising from its increased density, is in part counteracted and opposed by its tendency to the attracting body, and the pressure of all the superior strata; its sensible elasticity must be precisely the

New Series, VOL. I.

F

same as that of the surrounding calorific medium; for if it be greater, caloric will flow from the body into the surrounding medium; if less, to it; consequently, when a mass of matter is placed in a heated medium, it will continue to absorb caloric until the sensible elasticity of its calorific atmosphere is precisely equal to that of the surrounding caloric; under which circumstances both the body and the medium will have an equal effect upon the thermometer. The elasticity then of the caloric only can have any effect upon the thermometer; for whatever be the quantity absorbed by the body, the thermometer will experience the same degree of elevation, whether applied to the medium, or to the body itself.

Let now the mass of matter thus heated be brought into a medium of lower temperature: it is now evident, prop. 6, cor. and prop. 11, that the sensible elastic force of the calorific atmosphere will exceed that of the surrounding medium; the caloric will, therefore, have a tendency to diffuse itself: there are three different circumstances under which this may take place.

Case 1.—Suppose a warm solid to be brought into a medium of absolute cold, in vacuo, and entirely beyond the attracting force of any other form of matter; one force, viz. the elastic force of the surrounding caloric being removed, the calorific atmosphere of the body will have a tendency to enlarge its dimensions; and since the density of the several concentric strata is uniform, it must move in right lines, which meet in the centre of the body (supposed spherical), which radiation of heat will continue until the density of the lower strata is so far reduced as only just to balance the pressure of the superior rare ones, when the atmosphere becomes quiescent in all its parts; in this case, therefore, the radiation of caloric arises entirely from the motion into which the calorific atmosphere is put by the removal of the elastic force of the surrounding caloric. The caloric cannot leave the body, since there is no other form of matter by which it can be attracted.

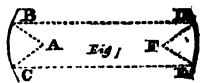
Case 2.—Suppose, that instead of being placed in absolute cold, the warm mass of matter is brought into a medium having a lower temperature than that to which the body was exposed; the other circumstances remaining, the calorific atmosphere will dilate itself, its parts moving in right lines, which will continue until the diminished density of the lower strata balances the elasticity of the surrounding caloric, and the pressure of its own superior strata, when the radiation ceases. In both these cases, the quantity of caloric contained in the atmosphere of the body is precisely the same; viz. that which it had at its highest temperature.

Case 3.—Suppose, in addition to either of the other cases, that masses of matter are placed within the calorific atmosphere, having a lower temperature than the parts of the atmosphere in which they are placed. The calorific atmosphere will have a

tendency to dilate itself, as before, and at the same time its quantity will be diminished by the attractive force of the matter in its vicinity; the atmosphere can, therefore, only become quiescent, when the requisite equilibrium of the two former cases is attained, and the surrounding bodies have arrived at the temperature of the parts of the atmosphere in which they are placed. During this operation, a current of heat must flow from the heated to the cooler mass, which, at its several distances from the heated body, will have the same density as the equidistant strata of the atmosphere. Hence the colder the body, which is absorbing the heat, is made, the more rapidly will the other lose its heat. Also of bodies equally cold, those which have the greatest capacities for heat cool the other more than those which have less capacity.

These three phenomena explain many of the primary properties of radiant heat. When bodies of equal magnitude and figure are heated to the same degree, then brought into a cooler medium, those which have the greatest attraction for heat, by prop. 6, cor. will be surrounded by atmospheres of the greatest density, and consequently at equal distances from the surface, will cause the greatest elevation of the thermometer; if black, and generally dark coloured bodies be supposed to attract heat more than those that have a lighter colour, this will exactly coincide with the observed phenomena; for if surfaces equally heated, and of equal magnitude and form, be one covered with lamp-black, another with red-lead, and another with tin-plate, at equal distances from these surfaces, the effects produced upon the thermometer will be nearly in the proportion of the numbers 100, 80, and 12 (Davy's Elements, p. 209); which is explained by supposing the attraction for heat of the lamp-black to be greater than that of red-lead, and this greater than that of tin-plate. It appears from prop. 6, cor. and prop. 11, that this will not be the order of their several capacities for heat; consequently, if equal and similar masses of the same matter be equally heated, and have their surfaces differently coloured, those which have the darkest colour will part with their heat in less time than those which are lighter coloured, or polished.

The cause of the reflection of heat may now be easily explained. Suppose A, fig. 1,



to be a heated body placed in a cold medium, its calorific atmosphere tends to enlarge its dimensions, or to dilate itself, moving in right lines from the centre of A. If it be placed in the focus of a parabolic mirror B C, which is of such a nature as to have feeble attraction for calorific, after impinging upon this surface, it will rebound, or be reflected; and since the angle of incidence is equal to the angle of reflection, by the property of the parabola, these incident calorific rays

will be reflected parallel to each other as B D, C E. If they fall upon another similar mirror D E, whose axis coincides with that of B C, they meet and are concentrated in its focus F : if, therefore, a thermometer be placed in the focus F, it will indicate a temperature superior to that of the surrounding medium, which process must continue until A is cooled to the temperature of the surrounding medium, when the radiation ceases. If A have the temperature of the surrounding medium, and F be a cold mass of matter, as a frigorific mixture, A will experience a diminution of temperature, which phenomenon has often been absurdly termed the radiation of cold, and is produced as follows : F, having a temperature below that of the surrounding medium, will absorb caloric from it, until the temperatures become the same. During this operation, F attracts towards its centre, which is the focus of the mirror D E, the caloric from the surrounding medium. Now it is easily proved from the principles of mechanics, that the particles of caloric being mutually repellant, the only rays that can impinge upon the mirror D E, so as to flow in succession to the focus, are such as make the angle of incidence equal to the angle of reflection, which property, from the nature of the parabola, belongs only to the parallel rays B D, C E, &c. whence, by reason of the attraction of F for caloric, it flows from the parts B C, &c. in parallel lines to D E, &c. and for the same reason, that the equilibrium of the medium may be preserved, must flow from A in divergent rays, impinging upon B C ; wherefore the temperature of A will be reduced. If, therefore, A be a delicate thermometer, it will indicate a temperature below that of the surrounding medium. These two phenomena of the radiation of heat are only modifications of one general principle: in the first, the caloric is put into motion, because the body is exposed in all directions to a medium, having a lower temperature than its own; in the second, to a medium which is cooler in only one of its parts.

If the mirrors be of such a nature as to absorb a considerable quantity of the incident caloric, the reflection of heat is in part destroyed ; for then the mirrors become heated, and instead of reflecting the heat in parallel lines, or to their foci, the caloric radiates from each point upon their surfaces, as from a centre ; consequently little or no effect will be produced in their foci. The same attains, if a thin transparent lamina be placed between them, which has a powerful attraction for, and readily absorbs, the incident caloric, which explains some phenomena which arise when plates of glass, &c. are placed between the mirrors.

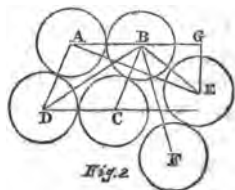
Crystallization.

It will be proper first to premise, that the particles which constitute a regular crystal must, at some previous time, have been separated from each other, either by the repulsive force of

heat alone, or by the agency of a liquid menstruum; and it is when these particles again approach to contact with each other that they arrange themselves in some particular and determinate order, in consequence of the operation of those forces which have been investigated. It is well known that crystals of precisely the same substance assume a great variety of external forms, arising from the external circumstances under which they are formed. We are perfectly ignorant of the causes which determine the external figure, and we can only expect to be able to show what external forms may result from a given arrangement of particles; and it is manifest that any external figure may result, in which every external particle is in a state of permanent equilibrium, or quiescence, which may easily be investigated by help of the foregoing propositions. There are, however, many peculiarities which cannot be examined until the nature of chemical union, and the structure of compound bodies, have been investigated.

Prop. 12.—If any number of equal and similar particles, influenced only by their inherent forces, mutually approaching each other, ultimately come into contact, they will be symmetrically arranged.

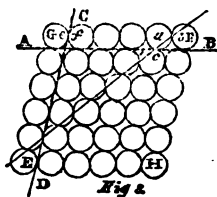
Let A, B, C, D, fig. 2, be four equal spherical particles of the same kind, which are preserved in contact by the force of cohesion, they will form a parallelogram (prop. 10). Let the two E, F, approach to D, C; they will join themselves in such a direction that a parallelogram will result, which is equiangular with the former; if not, let them unite differently; and by prop. 10, they must form the parallelogram C B E F, whose major diagonal D F is not parallel to that of A B C D: join A E, produce A D, and from E draw E G, perpendicular to A G. If A E represent the force with which A and E mutually attract each other, it may be resolved into the two A G, G E, of which, A G tends to bring E into contact with D, and G E to bring its centre to the line A G; and the same may be proved of the particle F; wherefore the particles E and F will, when they join D and C, have their centres in the straight lines A D, B C, produced, or the two parallelograms being equal will have their major and minor diagonals respectively parallel to each other.



Cor.—Hence crystals are always terminated by straight lines and plane surfaces.

Prop. 13.—Crystals will split so as to have a smooth fracture only in the directions of the rows of particles, and these directions are either parallel to, or make a determinate and constant angle with, each other.

Let E G F H, fig. 3, be a parallelogram formed of symmetrically arranged particles; if split in the directions A B, or C D, the surface will be smooth and plane, for the resisting force of cohesion is constant in every part of these lines, or their parallels, and each particle has to be separated only from one, as *a*, *c*, or *e*, *f*; but in any other direction, E F, each particle has to be separated from two, as *a* from *b* and *c*; therefore, the fracture cannot proceed according to that line, but must follow those directions in which it meets with the least resistance.



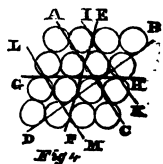
Cor. 1.—Hence, in an irregular fracture, all the irregularities are parts of parallelograms, having the constant angles E G F, or G F H.

Cor. 2.—Hence, by continual mechanical division, a number of nuclei may be obtained, which are all equiangular, and which are similarly situated in the original crystal.

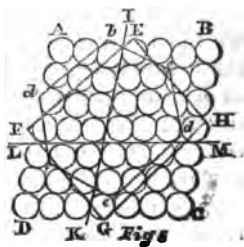
Cor. 3.—Since all the fractures of crystals, or the directions of their rows of particles, are parallel to, or form a constant angle with, each other, every base of a crystal may be considered as a parallelogram which wants some of its parts.

Cor. 4.—Since any number of such bases may be applied upon each other, which, by prop. 10, must follow the same law of arrangement as the bases, every solid crystal may be considered as formed from a parallelepipedon, similar to that which is obtained by the mechanical division of the crystal, by removing some of its parts.

We may now proceed to examine some of the external forms of crystals, and the first part to be investigated is the construction of the base, or that surface which results when a crystal is split, quite through its thickness: these are of two sorts; those which have some of their sides parallel to the directions of splitting, or to the sides of the original parallelogram, and those which have none of their sides so posited. Of those which have some of their sides parallel to those of their nucleus, the following will be some of the principal varieties: ABCD, fig. 4, being a parallelogram, composed of particles which are regularly arranged, E F and G H, which are parallel to its sides, are the directions of splitting, by prop. 13: the parallelogram is, therefore, one figure which the base of the crystal may assume. If the part A B C be removed, the external figure A D C is a triangle, whose angles are $D = \text{smaller angle of the figure}$; and $D A C$, $D C A$, each equal to half the greater angle: or if B C D be removed, the resulting triangular base has the angles A B' D, A D B, each equal to half the smaller angle of the nucleus, and



A the larger angle. Each of these will split into a number of smaller parallelograms, in lines parallel to $E F$, $G H$; and since the smallest visible portion of such a crystal is a parallelogram, equiangular with $A B C D$, having its sides parallel to the sides $A B$ and $B C$, either of the crystals $A D C$, $D A B$, may be considered as formed of such parallelograms, in which construction we observe the law of decrements of Haüy. If a smaller triangle, $I B K$, be removed, the resulting figure is an irregular pentagon, $A E K C D$, of which D is the less angle of the parallelogram A and C , each equal to the greater, $A I K$, $E K C$, each equal to the smaller + half the greater: or if a triangle less than $D C B$ be removed, there results a pentagon, of which the angles are the reverse of the above. If the triangles $E B K$, $L D M$, be removed, the resulting figure is an irregular hexagon, of which the angles A and C each equal the greater angle of the parallelogram, and the other angles each equal the less + half the greater. Other hexagons may be formed by removing, instead of the above, both the adjacent angles, or one of the above, and one of the adjacent ones. If all the four be removed, we have an irregular octagon; and in the same manner, several other figures may be formed, as trapezia, &c. The other class of the bases of crystals are the most abundant in nature: of these, the following are some of the principal forms: $A B C D$, fig. 5, being the parallelogram, if four triangles, $E B H$, $H C G$, &c. be removed, so that the straight lines $E H$, $H G$, &c. all meet each other, since the particles are all equal, the resulting figure $E F G H$ is a rectangle, by the nature of the rhombus; and its directions of splitting will be parallel to $I K$, $L M$, as before: this form



often occurs in nature. In all the above forms, the sides of the crystal have been parallel either to the sides or the diagonals of the original parallelogram; but there remains a variety of forms, in which this is not the case, of which the following is an example: $A B C D$ being the parallelogram, an irregular hexagon, $a b c d e f$, may be formed, and besides this, a great variety of quadrilateral, pentagonal, hexagonal, &c. figures after the same laws of decrement, in all which, every external particle is one of an entire elementary parallelogram; consequently, prop. 10, in a state of permanent quiescence. In all the above varieties, it is evident that these bases can only be split into equiangular parallelograms; and hence every form may be derived immediately from that figure, by circumscribing each circle by a parallelogram similar to $A B C D$, and having their corresponding sides parallel.

In the same manner we may proceed to examine the structure

of solid crystals, which are evidently formed by the application of the above bases upon each other; and in this, the particles follow the same order of arrangement as in the bases themselves. Thus in fig. 3, by the application of a number of equal and similar bases upon each other, is formed a rhomb: if the bases decrease continually in magnitude, the resulting figure is a quadrilateral pyramid. From fig. 5, results the cube, quadrilateral prism having equal sides, or the octohedron: from fig. 5, the hexagonal prisms and pyramids. The reason is now evident why the bases of different crystals are so variously inclined to their sides, for the base, which is obtained by splitting a crystal entirely through its thickness, is evidently formed by the union of regular rows of particles, prop. 13; so that having given the angles of the nucleus, its position in, and the number of sides of, the crystal, it is easy to determine its entire structure. It is obvious that what has been said applies only to crystals of simple matter, or such compounds as consist of particles which have the same magnitude. When the nature of combination has been examined, a still more numerous class of external forms will be exhibited, which, together with the former, will comprehend almost every variety found in nature, by the arrangement of spherical particles only. It may be objected, that the angles of crystals should undergo a change by the application of heat: we must, however, recollect that in the elementary parallelogram the smaller angle is just 60° at the absolute zero of temperature, in a simple substance, and 90° at most, when, on the point of fusing; therefore, only a very small change can be expected by even a very considerable variation of temperature; yet from certain experiments that I have made on this subject with some highly infusible matters, it is evident that this change does really take place; but from the great differences which exist in the cohesive force in the several parts of the same crystal, the effect is considerably irregular. The results of these experiments will appear at a future opportunity.

We may now see the reason why alum, most of the metals, and some other substances, which are formed in masses destitute of any symmetric appearance, by slow solution, &c. present an internal crystallization, as has been fully proved by the researches of Mr. Daniel.

(To be continued.)

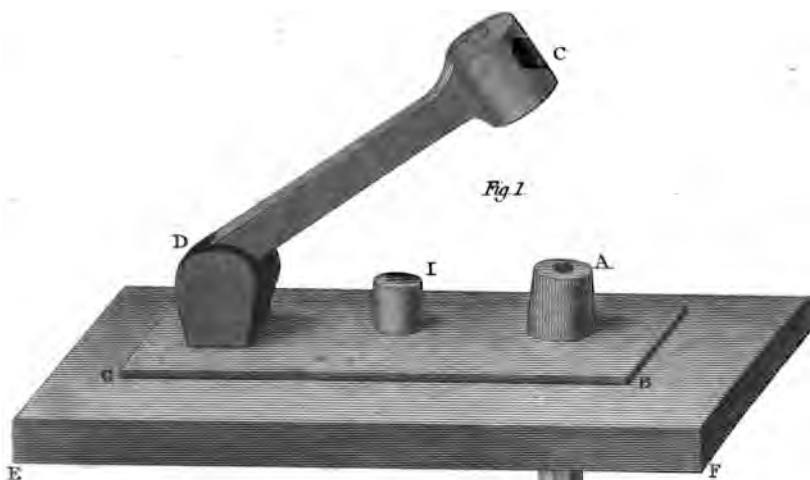


Fig. 1.

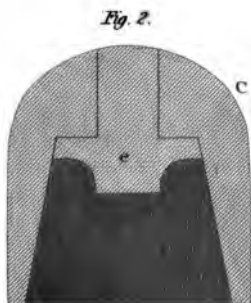


Fig. 2.

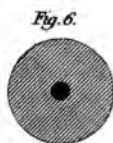


Fig. 6.

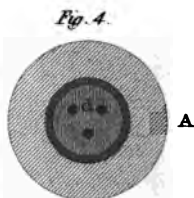


Fig. 4.

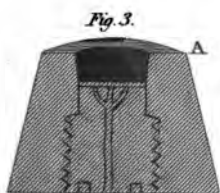
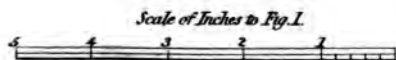


Fig. 3.



Fig. 5.



Scale of Inches to Fig. 1.

ARTICLE II.

*Explanation of an Apparatus, suggested by Col. Yule, for discharging Ordnance, upon Mr. Foreyth's Plan; and an Account of some curious Experiments performed with it.** By Mr. John Deuchar, Lecturer on Chemistry in Edinburgh. (With a Plate.)

It is proposed, in the present paper, first to explain an apparatus some time ago suggested by Col. Yule, for applying Mr. Foreyth's plan to the discharge of ordnance, without either the use of a light, or the usual prime; and secondly, to give a very brief account of several experiments which I have performed with it.

(I.) The apparatus has first to be noticed: it is very simple. It consists of a thick brass tube (see Plate III), fig. 1, A B, 15 inches long, which is meant to represent the touchhole of the gun; the diameter of the bore of this tube, fig. 6, is about the 18th or 20th part of an inch; it terminates at the top, A, in a cup, fig. 3, *d*, at the bottom of which the bore of the tube is divided into three very small holes, fig. 4, to prevent the powder falling into the wider tube: into this cup about one grain of the new composition, afterwards alluded to, is put, when the apparatus is to be used. At the top of the apparatus is a bar of brass, C D, which at the one end, D, turns upon a joint, and at the other, C, is supplied with a cap, in the centre of which is a steel projection or hammer, fig. 2, *e*; the cap covers the whole of the raised part, A, at the top of the tube, A B; and the hammer nearly fits the cup, fig. 3, *d*, and is made to strike flat upon the bottom of it, fig. 4, *d*. Between the joint of the bar, D, and the top of the tube, A, is placed a piece of cork, I, or any other elastic substance, to prevent the steel hammer, *e*, coming too close upon the composition before it is struck, and to make it spring back again after the discharge. The apparatus is united at the top by a frame of brass, G H, which, to prevent its being injured by the firing, is screwed upon another frame of wood, E F. The tube, A B, screws into four pieces; and it was into the hollows left, *a*, the joinings of these, *a*, *b*, and *c*, by only half screwing them, that the different substances we put which are noticed in the experiments.

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Explanation of Plate.

Fig. 1, represents the whole apparatus on a scale of 4-10ths to 1 inch.

Fig. 2, gives a section of the cap, C, and steel hammer, *e*, which strikes upon the powder at the top of the tube, A B.

* Read before the Wernerian Natural History Society, Dec. 18, 1820.

Fig. 3, gives a section of the top, A, of the tube, and the cup, *d*, for holding the powder.

Fig. 4, shows the bottom of the cup, which screws into A, from below, as shown in fig. 3; the hole through the tube from B to A divides into the three smaller openings.

Fig. 5, shows the end of one of the divisions of the tube, A B, and the size of the screw which unites them at *a*, *b*, and *c*.

Fig. 6, shows the bore of the tube at *a*, *b*, *c*, and B.

Figures 2, 3, 4, 5, and 6, show the parts of the apparatus of the full size; and for fig. 1, there is a scale given.

I may mention a few advantages of this mode of firing ordnance of every description. These are extracted from a communication on the subject, with which Col. Yule has very kindly favoured me. They are as follows:

"1. The instantaneous inflammation of the whole of the powder contained in the cartridge." The expansion of the aeriform fluids produced must act with greater force than in the old method: of course, less powder will be required for any proposed result; and as none of the charge is forced out uninflamed, any given quantity of gunpowder will give its fullest possible effect.

"2. The removal of all danger of explosions arising from the cartridges and loose powder coming in contact with lighted matches, in the gun-decks of men of war in time of action." The whole process is performed without the necessity of using a match-light of any description; nor is there any inflamed substance forced from the touch-hole during the discharge.

"3. The removal (in a great degree) of the inconvenience arising from the accumulation of smoke in the gun-decks of men of war, or in batteries in time of action." In the proposed method, there is not the slightest production of smoke at the touch-hole; and as neither prime nor prime tube is required, it is also free from any risk of accident by the discharge of these, and this at sea is a considerable advantage.

"4. The removal of all inconvenience arising from the priming being blown away by high winds, or washed off by heavy rains, or the shipping of a sea." The cap at the top of the new apparatus covers the touch-hole, and prevents any of these inconveniences of the old mode.

"5. A saving of the whole amount at present applied to the manufacture or purchase of quick-matches, priming tubes, flints, and various other articles now in use, both in the navy and in the field."

Another advantage of this mode deserves particularly to be noticed; namely, the rapidity with which the whole is performed. This facilitates the execution of a charge, by the effect being almost instantaneous with the pointing of the gun: it also saves the time at present spent in applying priming tubes, or trains of

powder ; for with Col. Yule's apparatus, the time that is required is very trifling.

(II.) Having now stated all that is necessary with regard to the apparatus itself, it remains for me, in the concluding part of this paper, to direct the attention of the Society to several experiments performed with it, which present rather extraordinary results.

At the request of Colonel Yule, I commenced, about the beginning of July last, a number of experiments, with the view of discovering a powder which should never miss inflaming the cartridge ; and I was fortunate enough to hit upon one. All the usual fulminating powders were first tried without success ; the fulminating mercury, when the fifth part of a grain only was used, rent asunder the steel plate at the top (marked *d*), and yet did not reach as far as the gunpowder placed at B. Some of the antimonial preparations were found sometimes to fire the gunpowder ; but they left a cake of crocus behind, which stoped up the holes, and could not be easily removed.

The following substances, and a variety of other inflammables, were used, mixed with an equal weight of superoxymuriate of potassa :

Golden sulphuret of antimony,	White lead,
Black sulphuret of antimony,	Litharge,
Red precipitate of mercury,	Orange lead,
Chinese vermilion,	Flowers of zinc,
Muriate of mercury,	Flowers of sulphur,
Red oxide of mercury,	Rosin,
Ethiop's mineral,	Gum arabic,
Red oxide of lead,	Gum gamboge.

None of these, however, produced any regular effect, and several of them gave no inflammation at all.

I shall now add a list of powders, all of which occasionally pierced the flannel at B, and inflamed the powder below it : very few of them, however, did so twice successively :

1. Superoxymuriate of potassa	5·0
Sublimed sulphur	2·0
Charcoal powder	1·0

This and No. 3 are the worst on the list.

2. Superoxymuriate of potassa	12·0
Flowers of sulphur	4·0
Charcoal powder	2·0
Gum arabic powder	1·0

This and No. 9 are fourth best.

3. Superoxymuriate of potassa	18·0
Flowers of sulphur	3·0
Gum arabic	1·0

4. Superoxymuriate of potassa	4.5
Flowers of sulphur.	1.5
Charcoal powder	1.5
Nitre	2.5

This and Nos. 5 and 8 are the third best.

5. Superoxymuriate of potassa	4.0
Flowers of sulphur.	1.0
Charcoal powder	1.0
6. Superoxymuriate of potassa	1.0
Sulphuret of antimony	1.0

This and No. 7 are fifth best, but they leave a hard cake of crocus of antimony, which adheres very strongly to the apparatus.

7. Superoxymuriate of potassa	1.0
Sulphuret of antimony	1.0

The proportions of this powder were taken by volume.

8. Superoxymuriate of potassa	2.0
Dried charcoal powder	1.0

9. Superoxymuriate of potassa	1.0
Dried charcoal powder	1.0

10. Superoxymuriate of potassa	3.0
Dried gunpowder	2.0

This is the best.

11. Superoxymuriate of potassa	1.0
Gunpowder, in fine powder	1.0

This is the second best.

After numerous trials with these, their comparative regularity in producing the desired effect was calculated to be as nearly as possible, as has been noted in the above list. But I found that even No. 10 was not so uniform as was necessary for Col. Yule's object. I had now, however, almost doubted of success, when it occurred to me to try the composition, which has since been found to succeed for more than 100 successive times, without leaving any residuum to stop the firing. Of the composition of this powder, and some cautions necessary to be attended to in its preparation, an account will be given in a future paper.

It was with this powder that I performed the experiments, which presented those striking results of which I have now to give a brief account.

I was led to these with the hope of elucidating still further than I had previously done a particular view of caloric, of which I have, for six years past, given an account in my chemical classes. I availed myself of the use of Col. Yule's apparatus to commence the investigation, and I am still occupied with it. At

present, therefore, it is only in my power to notice a few of the experiments in a detached form.

The first experiments were performed by firing the new composition, using about one grain, or rather less, at each trial, through a piece of cartridge flannel tied over the hole at the bottom, B, of the apparatus, when it inflamed a quantity of gunpowder fixed in a tin case below the flannel. This was repeated for many successive times without cleaning the apparatus, and the flame never failed to pierce the flannel and fire the gunpowder.

Should this succeed as regularly, when applied to the gun itself, there could remain no doubt but that it would possess all the proposed advantages. There was, therefore, fixed to a six-pounder, an apparatus similar to the one already described, except that it wanted the long tube, A B, for which the priming hole of the gun now became a substitute. It was charged with cartridge, and in several of the trials with ball and cartridge; and, upon the same experiments being repeated with it, it gave the same uniform results.

The next experiments were with the view of ascertaining how the results stood related to Sir Humphry Davy's theory regarding the impervious nature of wire gauze to flame. Wire gauze, of different degrees of fineness, was, in successive trials, put in the interior of the joinings, *a*, *b*, or *c*, of the tube, so as to cover the hole completely. When the coarser wire gauze was employed, the flame was found to pass through, and fire the gunpowder; but the same result never took place with wire gauze as fine as that used in Sir Humphry Davy's safety lamp, unless when the flame seemed to have burst a passage through the gauze. But when these experiments were performed without the flannel and gunpowder at the bottom, B, it was found that the flame went through even three pieces of the wire gauze at once.

The next experiments, and probably the most surprising of the whole, were with gunpowder placed in one of the divisions, *a*, *b*, or *c*, of the apparatus. In some of the trials, I found that the flame had passed through the gunpowder at *a*, *b*, or *c*, without inflaming it, although at other times I found it did not do so.

This at first appeared to be an objection to the proposed application of the apparatus. But, after repeated trials, I found that the above curious result only took place when the stroke with the hammer was slight; for when a smart blow was given, inflammation always took place.

In a few of the experiments, I put gunpowder at two divisions (*a* and *b*), and found that the flame sometimes went through both, without firing either portion; at other times one portion was inflamed, and one left unaltered.

In performing these experiments, I first put a small piece of

flannel upon the hole at the joining of the tube, and upon this I poured the gunpowder, using two, three, four, and sometimes five grains at once. After each trial, I found a scorched brownish mark in the centre of the flannel about the size of the hole of the tube.

A variety of experiments were also performed with flannel, paper, and other substances, placed between the joinings; in all which cases it was found that the flannel had been forced through, generally leaving a hole in the substance used; and a pale-coloured flame was observed to dart to a considerable distance below the bottom (B) of the tube.

In a future paper, I hope to have the honour of laying before the Society a fuller account of these curious experiments. In that paper I propose to enter upon the cause of the results which present themselves; and more particularly to attempt an explanation of that extraordinary one wherein we have the gunpowder remaining apparently inert to the flame which passes through it.

ARTICLE III.

Summary of the Magnetical and Meteorological Observations, during a Period of Three Years and Nine Months. By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR,

Bushey Heath, Stanmore, Jan. 1, 1821.

IN the *Annals of Philosophy* for May, 1820, you did me the favour of publishing a table containing the monthly mean variation of the magnetic needle for three years; and in the letter accompanying it, I expressed my opinion that the maximum of the western variation at this place occurred in the month of March, 1819. With the view of demonstrating that this conclusion had not been precipitately drawn, I continued the observations to the end of last year; and as the corresponding monthly mean variation in every case shows a diminution, I infer that the variation has been retrograde for the last 21 months.

In the latter part of the protectorship, the true and the magnetic meridians coincided. If $24^{\circ} 41' 42''$, the greatest variation, be divided by 162 (the number of years since that period), the quotient $9' 09''$ will be the mean annual increase; it is reasonable to suppose, therefore, that after the same lapse of time (from 1819) these meridians will again coincide; but by inspecting Table II. it will be seen that the mean annual decrease in lieu of being $9' 09''$ is $1' 57''$; consequently, an acceleration must take place, or the supposition is erroneous.

I remain, my dear Sir, very sincerely yours,

MARK BEAUFOY.

	1817.			1818.			1819.			1820.			Diff.			Diff.		
	Morn.	Noon.	Even.	Morn.	Noon.	Even.	Morn.	Noon.	Even.	Morn.	Noon.	Even.	+ 2-14'	+ 0-07'	+ 2-13'	+ 2-14'	+ 0-07'	+ 2-13'
April	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
May	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
June	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
July	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Aug.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Sept.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Oct.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Nov.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Dec.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
1818.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Jan.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Feb.	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
March	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24
	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24

N. B. The columns marked "diff." denote the increase and decrease of the variation in the same months of the different years.

TABLE II.—Table containing the Morning, Noon, and Evening Variation of the Magnetic Needle in 1818, 1819, and 1820.

1818.	Morn.	24° 34' 38"	1819.	Morn.	24° 33' 06"	1820.	Morn.	24° 31' 18"
	Noon.	24 43 26		Noon.	24 40 52		Noon.	24 39 04
	Even.	24 37 10		Even.	24 34 43		Even.	24 33 10

Mean Annual Decrease, 1' 57".

TABLE III.—Table containing the Mean Monthly Diurnal Variation of the Magnetic Needle, between Morning and Noon, and Noon and Evening, for Three Years and Nine Months.

Months.		1817, 1818.	1818, 1819.	1819, 1820.	1819, 1820.	Mean.
1817.						
April	Morn..	12' 51"	10' 44"	10' 33"	9' 31"	11' 00"
	Noon..	8 45	8 14	8 10	8 31	8 25
	Even..					
May	Morn..	10 15	9 31	8 40	9 26	9 28
	Noon..	7 50	7 14	7 12	7 08	7 21
	Even..					
June	Morn..	11 05	11 24	10 13	9 26	10 32
	Noon..	7 29	7 31	6 32	5 28	6 45
	Even..					
July	Morn..	10 52	10 35	9 41	10 19	10 22
	Noon..	6 23	6 45	6 35	5 34	6 19
	Even..					
Aug.	Morn..	11 35	11 18	10 16	9 35	10 41
	Noon..	9 06	8 08	8 25	6 46	8 06
	Even..					
Sept.	Morn..	8 34	10 53	9 06	9 13	9 27
	Noon..	6 58	7 54	8 06	7 30	7 37
	Even..					
Oct.	Morn..	9 40	7 52	6 41	8 33	8 11
	Noon..	—	—	—	—	—
	Even..					
Nov.	Morn..	6 06	8 17	6 01	5 15	6 25
	Noon..	—	—	—	—	—
	Even..					
Dec.	Morn..	3 59	4 16	3 51	3 31	3 54
	Noon..	—	—	—	—	—
	Even..					
1818.			1819.			
Jan.	Morn..	5 55	4 12	3 48	—	4 28
	Noon..	—	—	—	—	—
	Even..					
Feb.	Morn..	6 20	5 38	5 48	—	5 59
	Noon..	—	—	—	—	—
	Even..					
March	Morn..	8 19	8 24	8 46	—	8 30
	Noon..	7 50	6 25	5 48	—	6 41
	Even..					

TABLE IV.—Monthly Meteorological Table containing the Mean Height of the Barometer, Thermometer, and Hygrometer (De Luc's), at the Hour of the Morning, Noon, and Evening Observations of the Magnetic Needle. Also, the prevailing Winds, Quantity of Rain, Evaporation, and the Mean Heat of each Month, by a regular Series of Observation, with a Six's Thermometer.

Months	Barom.	Barom.	Barom.	Ther.	Ther.	Ther.	Hyg.	Hyg.	Hyg.	Wind	Rain.	Evap.	Mean temp.
	Inches.	Inches.	Inches.								Inch.	Inch.	
1817.	29-770	29-726	29-762	42-70	48-80	45-00	61-20	48-50	50-00	NE	—	—	—
April	29-286	29-269	29-286	50-1	57-4	50-2	58-2	52-9	53-6	SW	—	—	—
May	29-410	29-422	29-439	61-1	68-3	62-0	60-8	48-7	52-3	SW	—	—	—
June	29-368	29-612	29-349	56-7	65-3	59-4	64-3	55-2	59-2	SW	—	—	59-3
July	29-310	29-320	29-322	57-3	63-0	59-1	66-9	54-6	57-1	SW	—	—	58-9
Aug.	29-516	29-522	29-489	—	—	58-0	70-9	54-7	60-9	NE	—	—	58-1
Sept.	29-541	29-475	—	43-0	49-6	—	65-4	56-6	—	NE	—	—	44-6
Oct.	29-518	29-501	—	45-7	51-2	—	78-0	68-0	—	NE	—	—	47-7
Nov.	29-130	29-121	—	35-4	38-0	—	78-2	66-0	—	SW	—	—	35-9
Dec.	1818.												
Jan.	29-227	29-387	—	37-6	41-2	—	75-1	68-8	—	SW	—	—	37-9
Feb.	29-272	29-286	—	32-8	37-3	—	74-3	65-9	—	SW	—	—	34-9
March	29-154	29-132	29-427	37-6	43-6	39-8	66-1	57-2	51-4	SW	2-330	2-570	39-7
April	29-241	29-394	29-322	44-5	49-9	46-1	62-9	51-7	55-0	NE	3-750	2-590	45-8
May	29-393	29-463	29-423	51-7	59-0	53-4	52-2	42-1	47-1	NE	2-453	3-770	52-8
June	29-597	29-586	29-610	62-8	71-5	65-2	43-1	32-7	37-5	SW	0-330	6-980	64-0
July	29-629	29-621	29-625	65-8	73-3	68-1	43-8	33-8	37-7	NW	0-670	7-015	67-1
Aug.	29-625	29-605	29-611	62-3	68-8	63-1	41-3	30-4	32-2	NW	0-491	7-030	62-5
Sept.	29-346	29-326	29-357	54-9	62-2	58-1	61-1	44-6	51-1	SW	3-120	3-400	57-7
Oct.	29-411	29-391	—	51-6	57-9	—	70-4	57-1	—	SW	1-384	1-970	53-8
Nov.	29-394	29-357	—	46-5	51-4	—	77-7	65-2	—	SW	2-412	1-080	46-7
Dec.	29-611	29-607	—	35-6	39-8	—	79-5	65-5	—	NE	1-215	0-530	37-0
1819.													
Jan.	29-670	29-301	—	36-8	42-3	—	81-5	63-5	—	SW	1-906	1-400	38-9
Feb.	29-172	29-166	—	38-2	42-6	—	72-2	55-9	—	NW	2-826	1-430	38-8
March	29-386	29-682	29-333	42-0	46-8	46-3	65-3	52-2	51-4	NW	1-133	2-680	41-6
April	29-321	29-332	29-329	49-0	52-7	48-7	53-5	40-6	49-6	SW	2-468	3-440	48-5
May	29-440	29-435	29-489	54-8	61-1	55-5	48-9	37-3	44-2	SE	3-063	4-530	55-3
June	29-462	29-452	29-456	57-4	62-2	57-3	53-5	44-6	49-8	SW	1-950	4-250	57-2
July	29-233	29-539	29-556	62-1	68-7	63-0	57-0	44-9	52-2	NE	1-514	4-930	64-4
Aug.	29-599	29-579	29-566	62-8	70-9	65-1	66-9	48-6	60-0	NE	2-520	4-720	64-5
Sept.	29-511	29-518	29-519	56-7	62-9	58-1	79-2	64-7	67-5	SW	3-213	3-550	57-9
Oct.	29-370	29-362	—	47-7	53-1	—	79-9	68-9	—	NW	1-610	2-220	49-1
Nov.	29-253	29-233	—	37-4	42-0	—	79-9	72-8	—	NW	1-761	1-230	39-8
Dec.	29-243	29-288	—	32-4	36-4	—	82-9	75-7	—	SW	2-429	0-710	34-6
1820.													
Jan.	29-372	29-402	—	28-3	33-1	—	80-2	73-9	—	SW	1-020	0-360	31-3
Feb.	29-518	29-479	—	30-4	38-7	—	82-9	74-1	—	NE	1-143	0-765	35-6
March	29-408	29-439	29-295	37-6	44-5	46-8	74-2	63-6	67-4	NW	0-246	4-170	40-7
April	29-470	29-474	29-517	47-9	55-6	51-6	67-5	59-3	63-3	SW	1-505	3-750	49-5
May	29-375	29-362	29-339	51-9	58-8	53-8	66-4	58-6	63-4	SW	2-383	4-270	53-4
June	29-490	29-458	29-393	57-1	63-4	58-3	69-8	53-8	64-5	NW	1-724	4-390	58-1
July	29-519	29-530	29-458	60-3	65-7	60-5	69-1	60-1	67-5	NW	1-936	3-950	60-7
Aug.	29-461	29-468	29-443	59-3	65-6	60-1	70-5	59-8	63-4	SW	1-992	4-820	61-1
Sept.	29-550	29-543	29-595	53-8	61-1	56-3	71-2	57-9	61-1	SW	2-283	3-400	55-3
Oct.	29-200	29-166	—	45-6	51-4	—	71-8	61-9	—	NE	2-538	2-500	45-5
Nov.	89-371	29-371	—	39-7	44-0	—	76-7	70-0	—	NE	1-223	0-833	41-6
Dec.	29-467	29-449	—	36-6	39-2	—	75-9	72-5	—	NE	1-468	1-133	38-6

TABLE V.—*Annual Meteorological Table of the Mean Heights of the Barometer, &c. &c.*

Year.	Barom.	Barom.	Barom.	Ther.	Ther.	Ther.	Hyg.	Hyg.	Hyg.	Wind.	Rain.	Evap.	Mean heat.
	Inches.	Inches.	Inches.								Inches.	Inches.	
1818.	29.408	29.421	29.482	48.7°	54.3°	56.3°	59.2°	51.3°	44.6°	SW	—	—	50.0°
1819.	29.389	29.407	29.464	48.1	53.5	56.3	68.4	55.8	53.5	SW	26.415	35.150	49.3
1820.	29.433	29.426	29.434	45.7	51.8	55.3	73.0	63.0	64.4	SW	20.460	34.362	47.6

TABLE VI.—*General Table of Winds.*

Years.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	Var.
1818.	24	172	61	119	28	300	75	157	36	8
1819.	19	208	42	117	32	262	78	216	7	19
1820.	18	215	38	104	21	285	77	204	4	33
Mean	20	198	47	113	27	282	77	192	16	27

All the Winds *between* the cardinal points are described as N.E. S.E. S.W. and N.W. The number of observations in each year exceeded 850, but for readily comparing the results, the Table is calculated for 1000.

ARTICLE IV.

On a Method of applying Maclaurin's Theorem.

By Mr. James Adams.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Stonchouse, near Plymouth, Dec. 30, 1820.

CONSIDERING the following method of applying Maclaurin's Theorem (see his *Fluxions*, vol. ii. p. 198) as an improvement, I will thank you for its insertion in the *Annals of Philosophy*.

I am, Sir, your obliged humble servant,

JAMES ADAMS.

Problem.—Given $u = A \pm (Bz + Cz^2 + Dz^3 + Ez^4 + Fz^5 + \&c.)$

To find $\frac{du}{dz}$, $\frac{d^2u}{dz^2}$, $\frac{d^3u}{dz^3}$, &c. supposing dz constant.

By taking the successive differentials of the given equations, we have

$$u = A \pm (Bz + Cz^2 + Dz^3 + Ez^4 + Fz^5 + \&c.) = A \pm fz$$

$$\frac{du}{dz} = B \pm (2Cz + 3Dz^2 + 4Ez^3 + 5Fz^4 + \&c.) = B \pm f_1 z$$

$$\frac{d^2 u}{dz^2} = 2C \pm (2 \cdot 3 Dz + 3 \cdot 4 Ez^2 + 4 \cdot 5 Fz^3 + \&c.) = 2C \pm f_{11} z$$

$$\frac{d^3 u}{dz^3} = 2 \cdot 3 D \pm (2 \cdot 3 \cdot 4 Ez + 3 \cdot 4 \cdot 5 Fz^2 + \&c.) = 2 \cdot 3 D \pm f_{111} z$$

$$\frac{d^4 u}{dz^4} = 2 \cdot 3 \cdot 4 E \pm (2 \cdot 3 \cdot 4 \cdot 5 Fz + 3 \cdot 4 \cdot 5 \cdot 6 Gz^2 + \&c.) = 2 \cdot 3 \cdot 4 E \pm \phi z$$

$$\frac{d^5 u}{dz^5} = 2 \cdot 3 \cdot 4 \cdot 5 F \pm (2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 Gz + \&c.) \dots = 2 \cdot 3 \cdot 4 \cdot 5 F \pm \phi_1 z.$$

&c. &c.

Where $fz, f_1 z, f_{11} z, f_{111} z, \phi z, \phi_1 z, \&c.$ denote different functions of z .

Example 1.—Let it be required to develop $u = (a + z)^m$.

$$u = (a + z)^m = a^m + fz = A + fz \therefore a^m = A.$$

$$\frac{du}{dz} = m(a + z)^{m-1} = ma^{m-1} + f_1 z = B + f_1 z \therefore ma^{m-1} = B,$$

$$\frac{d^2 u}{dz^2} = m(m-1)(a + z)^{m-2} = m(m-1)a^{m-2} + f_{11} z = 2C + f_{11} z \therefore m(m-1)a^{m-2} = 2C,$$

&c.

Then by writing the value of $A, B, C, \&c.$ in the problem, we have $(a + z)^m = a^m + m a^{m-1} z + \frac{m(m-1)a^{m-2}}{1 \cdot 2} z^2$

$$+ \frac{m(m-1)(m-2)a^{m-3}}{1 \cdot 2 \cdot 3} z^3 + \&c.*$$

* The usual method of finding the values of $A, B, C, \&c.$ is to suppose the variable quantity in the given equations equal to nothing, by which means other equations are obtained which determine $A, B, C, \&c.$ in terms of the given function; thus in the equation $u = (a + z)^m = A + Bz + Cz^2 + Dz^3 + \&c.$ Suppose $z = 0$, then $a^m = A$,

$$\frac{du}{dz} = m(a + z)^{m-1} = B + 2Cz + 3Dz^2 + \&c. \text{ Suppose } z=0, \text{ then } m a^{m-1} = B,$$

$$\frac{d^2 u}{dz^2} = m(m-1)(a + z)^{m-2} = 2C + 2 \cdot 3 Dz + \&c. \text{ Suppose } z=0, \text{ then } m(m-1)$$

$a^{m-2} = 2C$ the same as before. This latter method is evidently as simple in its application as the preceding, but it does not appear to me to be so evident, particularly to beginners. If 0 be written for z in the equation $u = A + Bz + Cz^2 + Dz^3 + \&c.$

we should have $u = A$, a given quantity, therefore $\frac{du}{dz}, \frac{d^2 u}{dz^2}, \frac{d^3 u}{dz^3}, \&c.$ will be respectively equal to nothing.

Example 2.—Let it be required to develop $u = \frac{1}{a+z}$.

$$u = \frac{1}{a+z} = (a+z)^{-1} = a^{-1} + f_1 z = A + f_1 z \therefore A = \frac{1}{a},$$

$$\frac{d u}{d z} = -(a+z)^{-2} = -a^{-2} + f_2 z = B + f_2 z \therefore B = -\frac{1}{a^2},$$

$$\frac{d^2 u}{d z^2} = 2(a+z)^{-3} = 2a^{-3} + f_{11} z = 2C + f_{11} z \therefore C = \frac{1}{a^3},$$

$$\frac{d^3 u}{d z^3} = -2 \cdot 3(a+z)^{-4} = -2 \cdot 3a^{-4} + f_{111} z = 2 \cdot 3D + f_{111} z \therefore D = -\frac{1}{a^4},$$

&c.

Therefore, by writing the values of A, B, C, &c. in the problem, we have $\frac{1}{a+z} = \frac{1}{a} - \frac{z}{a^2} + \frac{z^2}{a^3} - \frac{z^3}{a^4} + \&c.$

Example 3.—To find the log. $(a+z)$ in a series, log. of base unity.

$$u = l(a+z) = l\left(1 + \frac{z}{a}\right) a = l\left(1 + \frac{z}{a}\right) + l a = l a + f_1 z = A + f_1 z \therefore l a = A,$$

$$\frac{d u}{d z} = \frac{1}{a+z} = \frac{1}{a} + f_1 z = B + f_1 z \therefore \frac{1}{a} = B,$$

$$\frac{d^2 u}{d z^2} = -(a+z)^{-2} = -a^{-2} + f_{11} z = 2C + f_{11} z \therefore -\frac{1}{a^2} = 2C,$$

$$\frac{d^3 u}{d z^3} = 2(a+z)^{-3} = 2a^{-3} + f_{111} z = 2 \cdot 3D + f_{111} z \therefore \frac{1}{a^3} = 3D,$$

&c.

By substituting for A, B, C, &c. we have

$$\log. (a+z) = l \cdot a + \frac{z}{a} - \frac{z^2}{2a^2} + \frac{z^3}{3a^3} - \frac{z^4}{4a^4} + \&c.$$

Example 4.—To find the log. $(a-z)$ in a series, log. of base being unity.

$$u = l(a-z) = l\left(1 - \frac{z}{a}\right) a = l \cdot a + f_1 z = A + f_1 z \therefore A = l \cdot a,$$

$$\frac{d u}{d z} = -\frac{1}{a-z} = -\left(\frac{1}{a} + f_1 z\right) = B + f_1 z \therefore B = -\frac{1}{a},$$

$$\frac{d^2 u}{d z^2} = -\frac{1}{(a-z)^2} = -\left(\frac{1}{a^2} + f_{11} z\right) = 2C + f_{11} z \therefore C = -\frac{1}{2a^2},$$

$$\frac{d^3 u}{d z^3} = -\frac{1}{(a-z)^3} = -\left(\frac{2}{a^3} + f_{111} z\right) = 2 \cdot 3D + f_{111} z \therefore D = -\frac{1}{3a^3},$$

&c.

By substituting for A, B, C, &c. we have

$$\log. (a - z) = l. a - \left(\frac{z}{a} + \frac{z^2}{2a^2} + \frac{z^3}{3a^3} + \frac{z^4}{4a^4} + \&c. \right)$$

Example 5.—Expand into a series the $\log. \left(\frac{1}{a+z} \right)$ $\log.$ of base unity.

$$\begin{aligned} u &= l \left(\frac{1}{a+z} \right) = l \left(\frac{1}{a} \right) + f z + A + f z \therefore A = l \left(\frac{1}{a} \right) \\ \frac{d u}{d z} &= - \frac{1}{a+z} = - \left(\frac{1}{a} + f_1 z \right) = B + f_1 z \therefore B = - \frac{1}{a}, \\ \frac{d^2 u}{d z^2} &= \frac{1}{(a+z)^2} = \frac{1}{a^2} + f_{11} z = 2 C + f_{11} z \therefore C = \frac{1}{2a^2}, \\ \frac{d^3 u}{d z^3} &= - \frac{2}{(a+z)^3} = - \left(\frac{2}{a^3} + f_{111} z \right) = 2 \cdot 3 D + f_{111} z \therefore D \\ &= - \frac{1}{3a^3}, \end{aligned}$$

&c.

Hence, and by substitution, we have

$$\log. \left(\frac{1}{a+z} \right) = l. \frac{1}{a} - \frac{z}{a} + \frac{z^2}{2a^2} - \frac{z^3}{3a^3} + \frac{z^4}{4a^4} - \&c.$$

Example 6.—Expand into a series the $\log. \left(\frac{1}{a-z} \right)$, $\log.$ of base unity.

$$\begin{aligned} u &= l \left(\frac{1}{a-z} \right) = l \left(\frac{1}{a} \right) + f z = A + f z \therefore A = l \left(\frac{1}{a} \right), \\ \frac{d u}{d z} &= \frac{1}{a-z} = \frac{1}{a} + f_1 z = B + f_1 z \therefore B = \frac{1}{a}, \\ \frac{d^2 u}{d z^2} &= \frac{1}{(a-z)^2} = \frac{1}{a^2} + f_{11} z = 2 C + f_{11} z \therefore C = \frac{1}{2a^2}, \\ \frac{d^3 u}{d z^3} &= \frac{2}{(a-z)^3} = \frac{2}{a^3} + f_{111} z = 2 \cdot 3 D + f_{111} z \therefore D = \frac{1}{3a^3}, \end{aligned}$$

&c.

Hence, and by substituting in the problem, we have

$$\log. \left(\frac{1}{a-z} \right) = l \left(\frac{1}{a} \right) + \frac{z}{a} + \frac{z^2}{2a^2} + \frac{z^3}{3a^3} + \frac{z^4}{4a^4} + \&c.$$

Example 7.—Expand into a series a^x , $\log.$ of base unity.

$$\begin{aligned} u &= a^x = \{1 + (a-1)\}^x = 1 + f x = A + f x \therefore A = 1, \\ \frac{d u}{d x} &= l a \{1 + (a-1)\}^x = l a + f_1 x = B + f_1 x \therefore B = l. a, \\ \frac{d^2 u}{d x^2} &= (l. a)^2 \{1 + (a-1)\}^x = (l. a)^2 + f_{11} x = 2 C + f_{11} x, \\ \therefore C &= \frac{(l. a)^2}{2}, \end{aligned}$$

$$\begin{aligned} \frac{d^3 u}{d x^3} &= (l. a)^3 \{1 + (a-1)\}^x = (l. a)^3 + f_{111} x = 2 \cdot 3 D + \\ f_{111} x \therefore D &= \frac{(l. a)^3}{2 \cdot 3}, \end{aligned}$$

&c.

By substituting for A, B, C, &c. we have

$$u = 1 + \frac{l \cdot a \cdot x}{1} + \frac{(l a)^2 \cdot x^2}{1 \cdot 2} + \frac{(l a)^3 \cdot x^3}{1 \cdot 2 \cdot 3} + \frac{(l a)^4 \cdot x^4}{1 \cdot 2 \cdot 3 \cdot 4} + \&c.$$

Example 8.—Expand into a series the $\log. \left(\frac{a+x}{a-x} \right)$ log. of base unity.

$$u = l \left(\frac{a+x}{a-x} \right) = l \cdot 1 + f x = 0 + f x = A + f x \therefore A = 0,$$

$$\frac{d u}{d x} = \frac{2 a}{a^2 - x^2} = \frac{2}{a} + f_1 x = B + f_1 x \therefore B = \frac{2}{a},$$

$$\frac{d^2 u}{d x^2} = \frac{4 a x}{(a^2 - x^2)^2} = 0 + f_{11} x = 2 C + f_{11} x \therefore C = 0,$$

$$\frac{d^3 u}{d x^3} = \frac{4 a}{(a^2 - x^2)^2} + \frac{16 a x^3}{(a^2 - x^2)^3} = \frac{4}{a^3} + f_{111} x = 2 \cdot 3 D + f_{111} x \therefore D = \frac{2}{3 a^3},$$

$$\frac{d^4 u}{d x^4} = \frac{48 a x}{(a^2 - x^2)^3} + \frac{96 a x^3}{(a^2 - x^2)^4} = 0 + f_{IV} x = 2 \cdot 3 \cdot 4 E + f_{IV} x \therefore E = 0,$$

$$\frac{d^5 u}{d x^5} = \frac{48 a}{(a^2 - x^2)^3} + f_V x = \frac{48}{a^5} + f_{VI} x = 2 \cdot 3 \cdot 4 \cdot 5 F + f_{VI} x \therefore F = \frac{2}{5 a^5},$$

&c.

By substituting for A, B, C, &c. we have

$$\log. \left(\frac{a+x}{a-x} \right) = 2 \left(\frac{x}{a} + \frac{x^3}{3 a^3} + \frac{x^5}{5 a^5} + \frac{x^7}{7 a^7} + \&c. \right)$$

By adding the series in Example 3 to the series in Example 6, or subtracting the series in Example 4, from the series in Example 3, we have the same result as above, which is evident from the nature of logarithms.

Example 9.—It is required to expand into a series the expression $\frac{a^m - x^m}{a - x}$.

$$u = \frac{a^m - x^m}{a - x} = a^{m-1} + f x = A + f x \therefore A = a^{m-1},$$

$$\frac{d u}{d x} = \frac{a^m - x^m}{(a-x)^2} + f_1 x = a^{m-2} + f_2 x = B + f_2 x \therefore B = a^{m-2},$$

$$\frac{d^2 u}{d x^2} = \frac{2(a^m - x^m)}{(a-x)^3} + f_2 x = 2 a^{m-3} + f_4 x = 2 C + f_4 x \therefore C = a^{m-3},$$

$$\frac{d^3 u}{d x^3} = \frac{2 \cdot 3 (a^m - x^m)}{(a-x)^4} + f_3 x = 2 \cdot 3 a^{m-4} + f_6 x = 2 \cdot 3 D + f_6 x \therefore D = a^{m-4}.$$

&c.

By substituting for A, B, C, we have

$$\frac{a^m - x^m}{a - x} = a^{m-1} + a^{m-2} x + a^{m-3} x^2 + a^{m-4} x^3 + \&c. \text{ Or,}$$

$$\frac{a^m - x^m}{a - x} = a^{m-1} \left(1 + \frac{x}{a} + \left(\frac{x}{a} \right)^2 + \left(\frac{x}{a} \right)^3 + \&c. \right)$$

Lemma.—To find the sines, cosines, tangents, &c. of circular arcs in functions of the arcs, radius being unity.

Given $\sin. z = \text{function of } z = f z$, then will

$$\cos. z = (1 - \sin.^2 z)^{\frac{1}{2}} = 1 - f_1 z,$$

$$\tan. z = \frac{\sin. z}{\cos. z} = \frac{f z}{1 - f_1 z} = f_{11} z,$$

$$\cot. z = \frac{1}{\tan. z} = \frac{1}{f_{11} z},$$

$$\sec. z = \frac{1}{\cos. z} = \frac{1}{1 - f_1 z} = 1 + f_{111} z,$$

$$\operatorname{cosec.} z = \frac{1}{\sin. z} = \frac{1}{f z}.$$

Example 10.—To find the sine of an arc in terms of the arc, radius unity.

$$u = \sin. z = 0 + f z = A + B z + C z^2 + D z^3 + \&c. = A + f z \therefore A = 0.$$

$$\frac{d u}{d z} = \cos. z = 1 - f_1 z = B - f_1 z \therefore B = 1,$$

$$\frac{d^2 u}{d z^2} = -\sin. z = -(0 + f_{11} z) = 2 C + f_{11} z \therefore 2 C = -0,$$

$$\frac{d^3 u}{d z^3} = -\cos. z = -(1 - f_{111} z) = 2 \cdot 3 D - f_{111} z \therefore 2 \cdot 3$$

$$D = -1;$$

&c.

By substituting for A, B, C, &c. we have

$$\sin. z = z - \frac{z^3}{1 \cdot 2 \cdot 3} + \frac{z^5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} - \&c.$$

Example 11.—To find the cosine of an arc in terms of the arc, radius unity.

$$u = \cos. z = 1 - f z = A - (B z + C z^2 + D z^3 + \&c.) = A - f z \therefore A = 1,$$

$$\frac{d u}{d z} = -\sin. z = -(0 + f_1 z) = B + f_1 z \therefore B = -0,$$

$$\frac{d^2 u}{d z^2} = -\cos. z = -(1 - f_{11} z) = 2 C - f_{11} z \therefore 2 C = -1,$$

$$\frac{d^3 u}{d z^3} = \sin. z = 0 + f_{111} z = 2 \cdot 3 D + f_{111} z \therefore 2 \cdot 3 D = 0,$$

$$\frac{d^4 u}{d z^4} = \cos. z = 1 - f_4 z = 2 \cdot 3 \cdot 4 E - f_4 z \therefore 2 \cdot 3 \cdot 4 E = 1,$$

&c.

By substituting for A, B, C, &c. we have

$$\cos. z = 1 - \frac{z^2}{1 \cdot 2} + \frac{z^4}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{z^6}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} + \&c.$$

Example 12.—To find the tangent of an arc in terms of the arc, radius unity.

$$u = \tan z = t = 0 + f z = A + f z \therefore A = 0,$$

$$\frac{d u}{d z} = 1 + t^2 = 1 + f_1 z = B + f_1 z \therefore B = 1,$$

$$\frac{d^2 u}{d z^2} = 2 t + 2 t^3 = 0 + f_{11} z = 2 C + f_{11} z \therefore C = 0,$$

$$\frac{d^3 u}{d z^3} = 2 + 8 t^2 + 6 t^4 = 2 + f_{111} z = 2 \cdot 3 D + f_{111} z \therefore D = \frac{2}{1 \cdot 2 \cdot 3},$$

$$\frac{d^4 u}{d z^4} = 16 t + 40 t^3 + 24 t^5 = 0 + f_4 z = 2 \cdot 3 \cdot 4 E + f_4 z \therefore E = 0,$$

$$\frac{d^5 u}{d z^5} = 16 + 136 t^2 + 240 t^4 + 120 t^6 = 16 + f_5 z = 2 \cdot 3 \cdot 4 \cdot 5 F + f_5 z \therefore F = \frac{16}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5},$$

&c.

From whence, and by substituting in the problem, we have

$$\tan z = z + \frac{2 z^3}{1 \cdot 2 \cdot 3} + \frac{16 z^5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \&c.$$

Example 13.—To find the secant of an arc in terms of the arc, radius unity.

$$u = \sec z = 1 + f z = A + f z \therefore A = 1,$$

$$\frac{d u}{d z} = t + \frac{t^3}{2} - \frac{t^5}{8} + \frac{t^7}{16} \pm \&c. = 0 + f_1 z = B + f_1 z \therefore B = 0,$$

$$\frac{d^2 u}{d z^2} = 1 + \frac{5 t^2}{2} + \frac{7 t^4}{8} - \frac{3 t^6}{16} \pm \&c. = 1 + f_{11} z = 2 C + f_{11} z \therefore C = \frac{1}{2},$$

$$\frac{d^3 u}{d z^3} = 5 t + \frac{17 t^3}{2} + \frac{19 t^5}{8} \pm \&c. = 0 + f_{111} z = 2 \cdot 3 D + f_{111} z \therefore D = 0,$$

$$\frac{d^4 u}{d z^4} = 5 + \frac{61 t^2}{2} + \frac{299 t^4}{8} \pm \&c. = 5 + f_4 z = 2 \cdot 3 \cdot 4 E + f_4 z \therefore E = \frac{5}{2 \cdot 3 \cdot 4},$$

$$\frac{d^5 u}{d z^5} = 61 t + \frac{241 t^3}{2} + \frac{641 t^5}{2} \pm \&c. = 0 + f_5 z = 2 \cdot 3 \cdot 4 \cdot 5 F + f_5 z \therefore F = 0,$$

$$\frac{d^6 u}{d z^6} = 61 + \frac{845 t^2}{2} \pm \&c. = 61 + f_6 z = 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 G + f_6 z \therefore G = \frac{61}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5},$$

&c.

$$\text{Hence we have } \sec z = 1 + \frac{z^2}{1 \cdot 2} + \frac{5 z^4}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{61 z^6}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} + \&c.$$

The student must understand that the developement of a function by the preceeding method is not always the most convenient; for instance, Examples 12 and 13 may be answered in a more simple and general manner by note N (with some slight alteration in the symbols), taken from the translation of Lacroix' Differential and Integral Calculus.

$$\text{Per trig: } \tan. x = \frac{\sin. x}{\cos. x} = \frac{x - \frac{x^3}{1.2.3} + \frac{x^5}{1...5} - \frac{x^7}{1...7} + \frac{x^9}{1...9} - \&c.}{1 - \frac{x^2}{1.2} + \frac{x^4}{1...4} - \frac{x^6}{1...6} + \frac{x^8}{1...8} - \&c.}$$

radius unity.

$$\text{By writing } a = \frac{1}{1.2}, b = \frac{1}{1.2.3}, c = \frac{1}{1...4}, d = \frac{1}{1...5}, e = \frac{1}{1...6}, f = \frac{1}{1...7}, \&c. \text{ the above equation becomes } \tan. x = \frac{x - b x^3 + d x^5 - f x^7 + h x^9 - \&c.}{1 - a x^2 + c x^4 - e x^6 + g x^8 - \&c.}$$

Now from a little attention to these equations, it will appear that $\tan. x$ may be represented by the series $A x + B x^3 + C x^5 + D x^7 + E x^9 + \&c.$

$$\text{that is, } \frac{x - b x^3 + d x^5 - f x^7 + h x^9 - \&c.}{1 - a x^2 + c x^4 - e x^6 + g x^8 - \&c.} = A x + B x^3 + C x^5 + D x^7 + E x^9 + \&c.$$

From whence we get

$$x - b x^3 + d x^5 - f x^7 + h x^9 - \&c. = \begin{cases} A x \\ (B - A a) x^3 \\ (C - B a + A c) x^5 \\ (D - C a + B c - A e) x^7 \\ (E - D a + C c - B e + A g) x^9 \\ \&c. \dots \dots \dots \end{cases}$$

Then by equating the coefficients of the corresponding powers of x on each side of the last equation, we have

$$A = 1,$$

$$B = A a - C = \frac{2}{1.2.3},$$

$$C = B a - A c + d = \frac{16}{1...5},$$

$$D = C a - B c + A e - f = \frac{272}{1...7},$$

$$E = D a - C c + B e - A g + h = \frac{7936}{1...9},$$

&c.

The law of continuation being evident, we, therefore, have

$$\tan. x = x + \frac{2 x^3}{1.2.3} + \frac{16 x^5}{1...5} + \frac{272 x^7}{1...7} + \frac{7936 x^9}{1...9} + \&c.$$

The same method may be applied to the developement of the cot. x .

For $\cot. x = \frac{\cos. x}{\sin. x} = \frac{1 - ax^2 + cx^4 - ex^6 + gx^8 - \&c.}{x - bx^3 + dx^5 - fx^7 + hx^9 - \&c.} = \frac{1}{x}$
 $-(Ax + Bx^3 + Cx^5 + Dx^7 + Ex^9 + \&c.),$ radius unity.

From whence we obtain

$$-ax^2 + cx^4 - ex^6 + gx^8 - \&c. = \begin{cases} (-A-b)x^2 \\ (-B+Ab+d)x^4 \\ (-C+Bb-A d-f)x^6 \\ (-D+Cb-Bd+Af+h)x^8 \\ \&c. \dots\dots\dots \end{cases}$$

Then by equating the homologous terms, we have

$$A = (a-b) = \frac{1}{3},$$

$$B = +Ab - (c-d) = \frac{1}{45},$$

$$C = -Ad + Bb + (e-f) = \frac{2}{945},$$

$$D = +Af - Bd + Cb - (g-h) = \frac{1}{4725},$$

&c.

Where the law of continuation is evident, we therefore, have

$$\cot. x = \frac{1}{x} - \left(\frac{x}{3} + \frac{x^3}{45} + \frac{2x^5}{945} + \frac{x^7}{4725} + \&c. \right)$$

In like manner we may develop the sec. x .

$$\sec. x = \frac{1}{\cos. x} = \frac{1}{1 - ax^2 + cx^4 - ex^6 + gx^8 - \&c.} = 1 + Ax^2 + Bx^4 + Cx^6 + Dx^8 + \&c. \text{ radius unity; from which equation we obtain the following, viz.}$$

$$0 = \begin{cases} (A-a)x^2 \\ (B-Aa+c)x^4 \\ (C-Ba+Ac-e)x^6 \\ (D-Ca+Bc-Ae+g)x^8 \\ \&c. \dots\dots\dots \end{cases}$$

Then by making the coefficients of $x^2, x^4, x^6, x^8, \&c.$ respectively equal to nothing, we shall have

$$A = a = \frac{1}{2},$$

$$B = Aa - c = \frac{5}{24},$$

$$C = -Ac + Ba - e = \frac{61}{720},$$

$$D = Ae - Bc + Ca - g = \frac{277}{8064},$$

&c.

The law of continuation being evident, therefore,

$$\sec. x = 1 + \frac{x^2}{2} + \frac{5x^4}{24} + \frac{61x^6}{720} + \frac{277x^8}{8064} + \&c.$$

We may from the same principle find the cosec. x in terms of the arc x , radius unity.

$$\text{cosec. } x = \frac{1}{\sin. x} = \frac{1}{x - bx^3 + dx^5 - fx^7 + hx^9 - \&c.} = \frac{1}{x} + Ax + Bx^3 + Cx^5 + \&c.$$

From whence we get

$$0 = \begin{cases} (A - b)x^2 \\ (B - Ab + d)x^4 \\ (C - Bb + Ad - f)x^6 \\ (D - Cb + Bd - Af + h)x^8 \\ \&c. \dots\dots\dots \end{cases}$$

Then by making the coefficients of $x^2, x^4, x^6, x^8, \&c.$ respectively equal to zero, we have

$$A = b = \frac{1}{6},$$

$$B = Ab - d = \frac{7}{360},$$

$$C = Bb - Ad + f = \frac{31}{15120},$$

$$D = Cb - Bd + Af - h = \frac{127}{604800},$$

&c.

The law of continuation being evident, therefore,

$$\text{cosec. } x = \frac{1}{x} + \frac{x}{6} + \frac{7x^3}{360} + \frac{31x^5}{15120} + \frac{127x^7}{604800} + \&c.$$

ARTICLE V.

On the Solution and Crystallization of Lime. By R. Phillips,
FRSE. FLS. &c.

MR. DALTON, in his "New System of Chemical Philosophy," has stated the curious fact that lime is more soluble in cold water than in hot, and has given the following table to show the difference which exists in water of various temperatures.

One part of water of	Takes up of lime.	Takes up of dry hydrate of lime.
60°	$\frac{1}{778}$	$\frac{1}{584}$
130	$\frac{1}{972}$	$\frac{1}{729}$
212	$\frac{1}{1270}$	$\frac{1}{952}$

"This table," he observes, "leads us to conclude that water at the freezing temperature would take nearly twice the quantity of lime that water at the boiling temperature takes."

Mr. Dalton has not attempted to account for the curious fact which he has discovered. Mr. Brande has not mentioned this

circumstance in his *Manual of Chemistry*, nor does Dr. Thomson or Dr. Henry attempt to explain the fact in their respective works, although they have given the results of Mr. Dalton's experiments. Dr. Murray, in the last edition of his *System of Chemistry*, observes, "this circumstance is extremely singular; augmented solubility from cold is contrary to all analogy, as well as to the principle on which the relation of temperature to solution depends."

With the intention of determining the comparative solubility of lime in water of different temperatures, and if possible also to discover the cause of the difference, I prepared some lime-water at the temperature of the atmosphere, which was then about 60° of Fahrenheit. A wine pint which had been filtered with as little exposure to the atmosphere as possible was decomposed by adding solution of carbonate of ammonia. The solution was examined with oxalic acid, and it appeared that the whole of the lime was precipitated by the carbonic acid of the carbonate of ammonia. The precipitated carbonate of lime being washed and dried weighed 17.3 grs. equivalent, according to Dr. Wollaston, to 9.7 grs. of lime; and as a pint of lime-water weighs about 7300 grs. it follows that water at 60° dissolves about $\frac{1}{758}$ of its weight, agreeing pretty nearly with Mr. Dalton's statement, and still more so with Dr. Thomson's, who finds that "758 grs. of cold water dissolve one of lime."

I next boiled some hydrate of lime in water, and filtered it with as little exposure to the air, and as rapidly as possible. A pint of this decomposed, as in the former case, by carbonate of ammonia, gave 10.5 grs. of carbonate of lime, equal, according to the scale, to 5.9 grs. of lime. Boiling water, therefore, as appears by this, dissolves only $\frac{1}{1280}$ of its weight of lime. I repeated this experiment, taking exactly 10,000 grs. of the lime water, the carbonate of lime weighed 14 grs. = 7.8 of lime; so that the hot water had taken up $\frac{1}{1280}$ of its weight, agreeing very closely with the former experiment, and as nearly with Mr. Dalton's as 1280 to 1270.

Some lime water was now prepared by putting hydrate into water, a very little above the temperature of 32°. This being filtered with the usual precaution, 10,000 grs. were decomposed in the manner already stated, and 27 grs. of carbonate of lime were obtained, confirming very nearly Mr. Dalton's opinion that water at 32° would dissolve twice as much lime as water at 212°: 27 grs. of carbonate of lime are equivalent to 15.2 of lime, and consequently water at near 32° dissolves $\frac{1}{840}$ of its weight of lime: to be exactly double, it should be $\frac{1}{420}$.

Having, by these experiments, satisfied myself of the correctness of the facts stated by Mr. Dalton, I proceeded to inquire into their cause. With this intention, I prepared some lime water at a little above 32° of Fahrenheit, and heated to ebullition 46 ounces in a flask, from which a long tube issued to prevent

the access of carbonic acid from the atmosphere. In a very short time, small white particles were deposited in the lime water, and after about two ounces of the water had been evaporated, I discontinued the ebullition, and cooled the lime water secured from atmospheric air.

On examining the particles which had been deposited, it was evident that they were crystalline, although the smallness prevented the determination of their form; they had nevertheless the usual brilliancy of saline crystals.

To determine what quantity of lime had been deposited by crystallizing, and what proportion it bore to the evaporation, I decomposed a pint of the lime water in the mode already described by carbonate of ammonia, the carbonate of lime precipitated was dried, and weighed 8.6 grs. = 4.85 grs. of lime. It is, therefore, evident that the action of the heat had caused the crystallization of the lime, and had effected it in a much greater degree than could be accounted for by the evaporation which occurred. The lime water before evaporation contained $\frac{1}{13}$ of its weight of lime; after 1-13th had been evaporated, the quantity of lime was reduced to $\frac{1}{13.5}$; so that more than one half of the lime was crystallized by evaporating 1-13th of the solution.

The cause of this crystallization appears to me to result from the effect which heat sometimes produces of increasing instead of diminishing the attraction of cohesion. The affinities which are brought into play are, the attraction of aggregation of the particles of the lime for each other, the attraction of the lime to form a hydrate with a small portion of water, and the mutual affinity existing between that hydrate and water of solution.

Among the cases which may be cited as proving the aggregating power of heat is this: If some peracetate of iron be decomposed by ammonia, the oxide is quickly redissolved by acetic acid; but if the oxide of iron be boiled in the solution from which it is precipitated, the acetic acid is incapable of dissolving it, on account of the cohesion which the oxide of iron has suffered by heating.

As crystallization is but a modification of cohesive affinity, we may, I think, consider, that the cohesive or crystalline affinity excited by the heat, increased by the affinity of the lime for a definite portion of water, is so much greater than the affinity of the hydrate of lime for the water of solution as to occasion crystallization. If the quantity of lime crystallized was in any degree proportional to that of the water evaporated, there would be no occasion to suppose the existence of the cause I have mentioned.

Mr. Dalton, instead of merely heating lime water, which had been prepared at a lower temperature, boiled it again with hydrate of lime; it is evident, therefore, that the crystallization which he would otherwise have observed could not be adduced to account

for the curious discovery which he had made. Although I was aware of the crystallization of lime which M. Gay-Lussac had effected by the agency of sulphuric acid and a vacuum; and after I had looked in vain to the authors whom I have quoted, in order to discover whether they were aware of the crystallization of lime which takes place by heating, I found that M. Thenard had stated the fact in his *Traité de Chimie*; however as he has not given any account of the extent to which it takes place, nor applied it to explain the greater solubility of lime in cold water, I have thought the details now given not altogether devoid of interest.

ARTICLE VI.

On the Bicarbonate of Ammonia. By Richard Phillips, FRSE. FLS. &c.

IN the seventh volume of the Journal of Literature, Science, and the Arts, I have given an analysis of the salt usually called subcarbonate of ammonia. I traced also the nature of the decomposition by which muriate of ammonia is converted by the action of carbonate of lime, first into sesquicarbonate, and eventually into bicarbonate of ammonia. This last salt I had not at that time seen produced, except by exposing sesquicarbonate of ammonia to the atmosphere. During the last summer, however, Dr. Henry gave me some salt which had been prepared with the intention of producing the common smelling salts. From some cause which has not been explained, the salt instead of being pungent had scarcely any smell whatever, and was on this account rejected by those to whom it was sent. This salt is more crystalline in its texture than the sesquicarbonate of ammonia: it does not become opaque by keeping; and turmeric paper held over it is scarcely affected by it even when fresh broken.

Dr. Henry informed me that he had found this salt to be bicarbonate, but not having given me the particulars of his analysis, I venture to state the results of my own experiments.

One hundred and twenty parts of the salt were dissolved in dilute sulphuric acid, the weight of the vial and acid being previously noted. The quantity of carbonic acid evolved amounted to 66.6 parts, equivalent to 55.5 per cent. An equal quantity of the same salt was dissolved in water, and added to a neutral solution of nitrate of lime, taking care that the latter salt was excess. Effervescence readily occurred, and by the application of a gentle heat, carbonate of lime was precipitated: this after being washed, was dried, and weighed $74.4 = 62$ per cent.

By Dr. Wollaston's scale, it will be seen that 62 of carbonate

of lime are equivalent to 21·16 of ammonia. One hundred parts of this salt consist, therefore, of

Carbonic acid	55·5	
Ammonia	21·16	leaving for
Water	23·34	
	<hr/>	
	100·00	

The exact composition and atomic constitution are as follow, and perfectly similar to the salt obtained by exposing what is usually termed subcarbonate (but correctly sesquicarbonate) of ammonia to the air.

Two atoms of carbonic acid	55·08	55·5
One atom of ammonia	21·5	21·7
Two atoms of water	22·64	22·8
	<hr/>		<hr/>
	99·22		100·0

ARTICLE VII.

On Rain-Guages. By Mr. Richard Davenport.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Jan. 10, 1821.

I PERCEIVE in your last number another letter added to the many that have appeared on the subject of the different indications of similar rain-guages differently situated. It is rather surprising that the writer should not before have met with the observation that a greater quantity of rain falls on a given area on the surface of the ground than on an equal area at a considerable height above it; it being noticed and accounted for in elementary books and lectures on meteorology generally.

It is more surprising that disputes should have arisen and have been carried on from time to time in a scientific publication, whether, on a given area, an equal or a smaller quantity of rain drops would be received if falling obliquely, than when falling perpendicularly, *ceteris paribus*; it being a subject of easy demonstration, and probably by the greater part of your readers perceived to be so, although some of the disputants seem to continue in their first error.

There is, however, a cause of the different indications of similar rain-guages under the circumstances that have led to the discussion, which I have not seen any where noticed, and it is for this reason I trouble you with this letter.

If we suppose rain drops to proceed at uniform relative dis-

tances from each other out of a cloud of given magnitude extending horizontally over a certain area of land, whether those rain-drops fall perpendicularly, or proceed with a motion derived from a projectile force received in the upper regions, the lines described in their fall will be parallel to each other, and the space they fall on on the surface of the earth will be in either case equal to the area of the cloud: in one case, immediately under the cloud; in the other, at some distance from it. In either case, similar rain-gauges would receive equal quantities with each other. This cannot admit of serious dispute; but when we consider that the oblique fall of rain-drops is given them by the motion of the medium through which they pass, and that when that medium (the wind) with its rain-drops impinge against any opposing surface, the rain-drops are retained by that surface, while the volume of wind proceeds discharged of that rain, the case is altered; for it becomes evident that an eddy of reverberated wind actually contains a smaller quantity of rain than is contained in an equal volume of uninterrupted wind.

Suppose now a wall extending on a plain, and the rain in its oblique fall beating against the face of this wall, and two rain-gauges, placed one on each side of the wall at the base, and a third on the top. If the oblique direction of the rain were owing to an original projecting force, one of those at the base would receive no rain at all; but the other, and the one at the top, would receive equal quantities with each other (putting out of question the trifling difference of *accumulation* of drops falling through the small perpendicular space between them) and those quantities would be equal to those of similar gauges placed on the uninterrupted plain. But since the oblique direction is owing to the horizontal motion of the stratum of air through which the rain passes, we ought to expect the upper gauge to receive less; for the volume of air that beats against the wall is not annihilated, but must rise; and passing over the wall passes over the upper gauge discharged of rain, driving away an equal volume of saturated air. We may also expect the one at the base even at the windward side to receive less than others at a distance from the wall, for part of the wind must be reverberated over that. In short there will be a general mixture and confusion of saturated and emptied volumes of air disturbing the indications of all the gauges within the influence of the eddies of wind.

Take another case.—Suppose an extended plain abruptly broken by a perpendicular cliff into two different levels. Say, a cliff running from north to south, and on the west side of it a high level plain, and on the east side a plain on a lower level. Place a rain-gauge on the edge of the cliff. Suppose a driving rain with a *west* wind. This gauge will receive its due quantity of rain equally with others on the open plain. But suppose a

driving rain with an *east* wind. In this case it ought to receive much less, because the face of the cliff will receive all the rain contained in the wind driven against it, and will drive upwards the wind discharged of rain. If the wind drives up a slope, instead of a perpendicular wall or cliff, the same thing will happen in a less degree; and all irregularities of surface will more or less disturb the equality of distribution of rain; and that in different degrees under different directions of the wind. It is also evident that as the rain driven away by the eddies must fall somewhere, it will fall in increased quantities in irregular portions elsewhere, and this will double the difference of indication of gauges at no great distance from each other.

I am, Sir, your obedient servant,

RICHARD DAVENPORT.

ARTICLE VIII.

On the Machine for measuring a Ship's Way by the Log Line.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

THE enclosed communication I received from a gentleman of great respectability in Denmark. You will oblige me by inserting it in the *Annals of Philosophy*.

Very truly yours,
G. FORCHHAMMER.

Nothing is more probable than that two different persons may have conceived similar ideas on any particular subject; and when the inventions to which they give rise prove of essential utility, it is but just that each should enjoy the honour that really is due to him.

It is stated in the *Journal of Science and Arts*, edited at the Royal Institution of Great Britain (vol. ii. p. 90), that Mr. Newman has claimed to be the first inventor of a machine for measuring a ship's way by the log line. This is stated to be effected by means of a sort of watch which is to serve on board of ships in lieu of the usual sand glasses of one-quarter, one-half, or one whole minute.

Mr. Newman's description of his machine is as follows :

"The quarter, and half minute glasses in general used for measuring a ship's way have always been found very irregular. This irregularity arises from various causes, and particularly the state of the atmosphere at different times, and in various latitudes; and even when they are new, it is scarcely possible to find two that will run out in the same time.

"As the log line is in general use for measuring a ship's way, it is evident that so inadequate a method of ascertaining the portion of time required must introduce many errors into the estimate made of the velocity and progress of the vessel; and a small, simple, and correct machine that can be depended upon for this purpose appears to be a very desirable improvement.

"A small machine which I have invented, and which has had the decided approbation of many naval officers, appears to possess every requisite for the purpose to which it is intended to be applied. It is enclosed in a round brass box, three inches and a half in diameter, and one and a half in depth. It has a dial, the circumference of which is divided into 60 parts. In the centre is an index, which is carried round by the machine once in 60", or one minute. At the 15th, 30th, 45th, and 60th second, are holes made in the dial through which pins are pushed up or down by small buttons on the outside. The dial is covered in by a strong glass.

"When the machine is used, being wound up, the index is to be retained at 60" by putting the pin up at that division. If then 15" are to be counted, the pin at 15" is to be put up, and the moment the log is delivered, that at 60" depressed; the index immediately advances, and continues in motion until stopped at 15". If 30", or 45", or 60", are to be told, the pin belonging to the number required is to be put up, and the time told as before.

"The beats of the machine can be heard at a considerable distance, and the moment at which it stops so readily distinguished that it may be used as well in a dark night as during day, or by a light; and as it is perfectly accurate, very strong and very portable, it seems well adapted to supply the place of those incorrect minute glasses at present in use aboard on all ships."

I think it, however, a duty incumbent on me to observe, that Rear-Admiral Lowenorn (a gentleman who as a hydrographer, in his capacity as Chief Director of the Royal Marine Chart Archives, has been of essential service to mariners; and besides this, has caused great number of lighthouses to be erected upon the Danish coast, &c. &c.) had already conceived this very idea several years before, and was the immediate cause of the construction of a watch by an artist, by the name of Sparrevogn, who in the year 1804 constructed a portable log watch in the form of a common watch, which the person employed in logging may conveniently carry about his neck, suspended by a ribbon, or in any other manner. In the very moment, the first mark of the log line has run out of the man's hand, he needs only to press upon a spring of the watch, which directly sets it going, and the index shows exactly the full seconds. When the index of the watch points on to the 14th second (which serves to denote one-quarter of a minute), this watch strikes a bell loudly in the

inside, so that the person who is logging may directly, and in the very same moment; stop the line and count the knots; the watch meanwhile continues to go, and the index indicates the seconds until the 28th second (for this division is adopted here for the computation of our log), when the watch again gives a loud stroke, and the man at that very moment stops the line, in case he has let the log run to that moment which is used when the ship runs at a moderate rate; the watch is now run down, and stops at once. It is not wound up in any other manner: this is effected by the pressure upon the spring by which it repeatedly may be set going.

The contrivance appears to be somewhat superior to that described by Mr. Newman. Mr. Sparrovogn (the above-mentioned Danish watch-maker, who is now dead) made several of these log-watches. The Danish Board of Admiralty rewarded Mr. Sparrevogn with a proper remuneration, and Admiral Lowenorn laid before the Royal Society of Sciences at Copenhagen (of which he is a Fellow) a drawing of the construction of this watch, together with an explanatory description of it, which met the approbation of the Society.

ARTICLE IX.

On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints which many of them develop on Exposure to a polarised Ray. By J. F. W. Herschel, Esq. FRS. Lond. and Edin.*

SINCE the period of the brilliant discovery of Malus of the polarisation of light by reflection, the investigation of the general laws which regulate the action of crystallized bodies on light has advanced with a rapidity truly astonishing, and the labours of an Arago, a Brewster, and a Biot, have already gone far towards completing the edifice of which that distinguished philosopher laid the foundation. When Malus wrote, the list of doubly refracting crystals was small, and the most remarkable among them possessing only one axis of double refraction, it seems to have been for some time, tacitly at least, presumed that the law discovered by Huygens, and since re-established in the most rigorous manner for that one,† might hold good in all.

* Read before the Royal Society of London, Dec. 23, 1819.

† The author of the article on Polarisation, in the 63d number of the Edinburgh Review, just published, is guilty of a most unpardonable mistake, in asserting (p. 188), as deducible from Dr. Brewster's experiments, that the Huygenian law is *incorrect*, for carbonate of lime. Dr. Brewster's general formulae for crystals with two axes resolve themselves into the Huygenian law when the axes coincide, of which case it is only an extension. That excellent philosopher, if I understand English, in the paragraph which gave rise to this strange assertion, only means to declare his opinion that it remains undemonstrated.

The discovery, by Dr. Brewster, of crystals possessing two axes of double refraction, or two directions in which a ray may penetrate their substance without separation into distinct pencils, has proved the fallacy of any such generalization, and rendered it necessary to enter on a far more extensive scale of investigation.

There are two methods which may be pursued in observations on double refraction and polarisation, the one direct, the other indirect. The former turns on immediate observations of the angular deviation of the extraordinary pencil, and is, of course, only applicable when the forces which act exclusively on the rays composing it are sufficiently intense to cause a sensible separation of the two pencils. There exist, however, a multitude of crystals in which the force of double refraction is so feeble as to produce scarcely any, or at most a very inconsiderable deviation of the extraordinary ray, and in which, consequently, the laws of double refraction could neither be investigated nor verified, without having recourse to some artificial means of magnifying the quantity to be observed; a thing easy enough in theory, but requiring, in practice, the greatest nicety on the part of the observer, and in many cases altogether impracticable, from the physical constitution of the crystals themselves. The indirect method depends on the discovery of Arago, scarcely inferior in intrinsic importance to that of Malus, of the separation of a polarised ray into complimentary portions by the action of a crystalized lamina. It was reserved, however, for the genius of M. Biot to trace this striking phenomenon to its ultimate causes, in the action of crystals on the differently coloured rays, and to develop, in a simple and elegant theory, the successive gradations by which the polarisation of a ray in its passage through a doubly refracting crystal is performed; while, on the other hand, the splendid phenomena of the polarised rings, which we owe to Dr. Brewster, have established the connection of the tints so polarised with the force producing the deviation of the extraordinary pencil, and shown the legitimacy of conclusions respecting the intensity of the latter, drawn from observations on the former.

This indirect mode of observation, which consists in noticing the gradations of colour for different positions and thicknesses of the crystal, possesses three capital advantages. The first is its extreme sensibility, which enables us to detect the existence, and measure precisely the intensity of forces, far too feeble to produce any measurable deviation of the extraordinary pencil. It, in fact, affords the rare combination of an almost indefinite enlargement of our scale of measurement, with a possibility of applying it precisely to the object measured, arising from the distinctness of all its parts. Another, no less precious, is the leading us by mere ocular inspection to the laws of very complicated phenomena, and enabling us to form, and mould, as it were, our analytical formulæ, not on a laborious, and sometimes

deceptive discussion of tabulated measures, but on the actual form of the curves themselves, which are loci of the functions under consideration. It is true, that a reference to tabulated measures is indispensable to give precision to such first approximations; but the power this mode of observation affords of copying our outline fresh from nature, and from the general impression of the phenomena, brought at once under our view, is an advantage not to be despised. Nor ought we, lastly, to omit, in our estimate of advantages, the means thus afforded us of subjecting the minutest fragments of a crystal to a scrutiny as severe as the most splendid specimen, and thus extending our researches to an infinitely greater variety of natural bodies than we could otherwise hope to examine.

In order, however, to render observations on the tints developed by polarised light available, they must be comparable to each other; and it, therefore, becomes an object of the first importance to ascertain the existence, and discover the laws, of any causes which may operate to disturb their regularity. Ever since I first engaged in experimental inquiries on the polarisation of light, I was struck by the very considerable deviation from the succession of colours in thin laminæ, as observed by Newton, which many crystals exhibit when cut into plates perpendicular to one of their axes. I at first attributed this to a want of perfect regularity in their structure, or to inequalities in their thickness, arising from my own inexperience in grinding and polishing their surfaces; and it was not till habit had rendered me familiar with all the usual causes of deception, that, finding the same phenomena uniformly repeated in different and perfect specimens, my curiosity became excited to inquire into their cause, the more so as they now began to assume the form of a radical and unanswerable objection to the theory of M. Biot, above alluded to, which affords so perfect an explanation of the tints in crystals with one axis.

These phenomena have not escaped the vigilance of Dr. Brewster. In his paper of 1818, he distinctly notices the fact of a deviation from Newton's scale, in crystals with two axes, and promises a more detailed account of it, which, however, has not yet appeared. But the object of the present communication is not thereby anticipated, as in the only passage in that paper in which he expresses himself otherwise than obscurely on its cause, he appears to regard the deviated tints as analogous to those developed along the axis of rock crystal and by certain liquids; an analogy which, in the present state of our knowledge on that perplexing subject, it seems not easy to admit. In a paper too, which has lately appeared, containing the interesting observations of the same excellent philosopher on the optical structure of the apophyllite, he remarks the very striking deviation of the colours of this crystal from Newton's scale "in the

first orders" of its rings; and while he remarks that such deviations are common enough, and indeed universal in crystals "in which the rings are formed by the joint action of two axes," seems to think this analogy close enough to authorize the substitution of two rectangular axes of a negative character for the single positive axis actually observed, according to his own peculiar and ingenious views on this subject. I lost no time in endeavouring to procure a specimen of this mineral, and by the kindness of my friend, Sir Samuel Young (to whom I owe more than one obligation of this nature) was favoured with one sufficiently transparent for optical examination. From my observations on this body, I think I shall be able to demonstrate satisfactorily that the phenomena of the apophyllite depend on a principle distinct from that which produces the chief part of the deviation of tints in most crystals with two axes.

The course I propose to pursue is, first, to describe the phenomena themselves. I shall then show how these phenomena, complicated as they are in appearance, are all reducible to one very simple and general fact; viz. that the axes of double refraction differ in their position in the same crystal for the differently coloured rays of the spectrum, being dispersed in one plane over an angle more or less considerable, according to the nature of the substance. In many bodies, the magnitude of this dispersion of the axes is comparatively trifling, while in some, not otherwise remarkable for a high ordinary or extraordinary dispersive power, it is enormous, and must render all computation of the tints in which it is not taken into consideration, completely erroneous; and indeed obliterating almost every trace of the Newtonian scale of colour. We have here then a new element, which, for the future, must enter into all formulæ of double refraction pretending to rigour, and at the same time are presented with another very striking instance of the inherent distinction between the differently coloured molecules of light, which, since the time of Newton, every new step in optical science has tended to place in a stronger point of view. At the same time, by the easy and complete explanation this principle affords of all the more perplexing anomalies in the tints, the theory of alternate polarisation to which they were hitherto so palpable and formidable an objection, stands relieved from every difficulty, and may now be received as fully adequate to the representation of all the phenomena of the polarised rings, and entitled to rank with the fits of easy transmission and reflection, as a general and simple physical law. In fact, if we investigate by this theory a general analytical expression of the tint developed for any position and thickness of the plate, taking this element into consideration, it will be found to include all the phenomena, as far as they can be computed; while the law of dispersion remains unknown. But we may go yet further. The

nature of the formula furnishes an equation by which the actual quantity of the separation of the extreme red and violet axes may be deduced from observations of the tints of a very simple and accurate nature, being perfectly analogous in principle to the "method of coincidences," which has of late been applied with such success to the most delicate investigations in every department of physical science. The comparison of the results afforded by that equation with those deduced by direct observation on homogeneous light; while it leaves nothing to desire in point of accuracy, leads to another important result, viz. that the proportionality of the minimum lengths of the periods performed by differently coloured molecules, in a doubly refracting crystal, to the lengths of their fits of easy reflection and transmission, supported as it is by an induction of no ordinary extent and accuracy, is yet not universal, admitting a deviation to a very large amount. Hence must of course arise a kind of secondary deviation in the scale of tints. In crystals with two axes, however, this is masked by the much more powerful effect of the separation of the coloured axes; yet even there, is not altogether insensible in an extreme case. In the apophyllite, however, the agency of this secondary cause is placed in the fullest evidence. The application of our general formula to the anomalous tints of that body, while it proves incontestably the exact coincidence of the axes for all the coloured rays, points out at the same time a peculiarity in its action on the more refrangible extremity of the spectrum, of a nature so singular, so entirely without example in all the multitude of natural and artificial bodies hitherto examined, as to render me extremely desirous of prosecuting the research, with the aid of more perfect specimens and improved methods of observation.

Having arrived at the general result of a dispersion of the axes by the sole consideration of the gradation of tints in plates of various thicknesses, it becomes interesting to verify it by direct and independent observation. This I have accordingly done; and the fortunate discovery of a substance in which it is of enormous magnitude, puts it in our power to render the fact sensible to the eye of the most unpractised observer, by an exceedingly simple experiment, to be described in its place.

II. *Of the general Phenomena of Crystals which develop Tints deviating from Newton's Scale, by Exposure to polarised Light.*

In describing the phenomena, I shall at present confine myself to the tints developed along the principal section of the crystal, which is supposed placed in an azimuth 45° with the plane of primitive polarisation. The observations of the tints in this position are most easily made, and least liable to error, and we shall see presently that it would be superfluous as well as embarrassing to examine other situations, the law of the phenomena being completely deducible from this. In this series of observa-

tions, then, we traverse the polarised rings (Pl. IV.) fig. 1,* in the direction of their axis of symmetry $A A'$, passing through their poles P, P' , and centre O . Now if we subject to this examination any one of the following substances :

Sulphate of soda?
Sulphate of baryta,
Nitrate of potash,

Arragonite,
Sugar,
Hyposulphite of strontia,

it will be seen that the tints between the poles $P P'$ correspond to lower orders of colour than would result from assuming P, P' , as the origin of the scale, and agree much better with the assumption of certain points p, p' , without the poles, as their zero, or commencement of the scale. The poles themselves too instead of being absolutely black, are tinged with colour; and the tints beyond them, instead of descending in the scale from the poles outwards, continue to rise till they reach their maximum (which is a white, more or less brilliant, or an absolute black) at the points p, p' ; after which they descend again to infinity. Not that in any case they coincide precisely with the scale of Newton, even with this correction, but, except in extreme cases, approximate to it within some moderate limit of error.

If, on the other hand, we examine in the same manner one of the following bodies :

Tartrate of soda and potash,
Borax,
Mica,

Sulphate of magnesia,
Topaz,

it will be found that the imaginary points, p, p' (which we shall call the *virtual poles*), from which the tints must be reckoned inwards and outwards, to produce the nearest possible agreement with Newton's scale, lie between the poles P, P' .

In all these crystals, as the thickness of the plate examined increases, the virtual poles $p p'$ recede from the actual ones $P P'$, at least in respect of the number of alternations of colour which intervene between them: in other words, the tint developed in the poles, or along the apparent axes of the crystal, descends in the scale of colour, as the thickness of the plate increases, and *vice versa*. In very small thicknesses, the tints approximate pretty closely to Newton's scale, or wholly coincide with it, while in very great ones, the tint developed in the poles is the composite white of the extremity of the scale. The angular distance, however, of the virtual poles from each other and from the axes, remains absolutely unchanged for all thicknesses; and this striking fact, which I have proved by numerous and satisfactory experiments, was first suggested for examination as a result of theory, and would equally hold good, as will presently be proved, for every conceivable law of double refraction.

* The whole of the figures in this paper not being referred to in the present part, the Plate will be given entire, with the remainder of the paper, in the next number.—Ed.

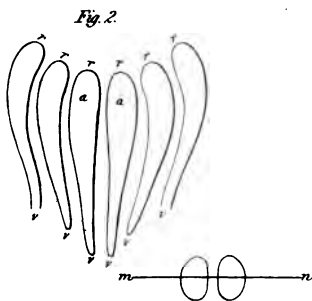
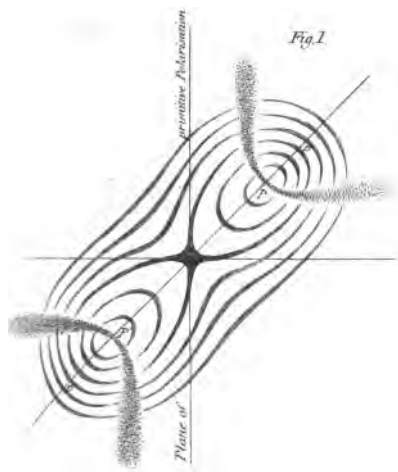
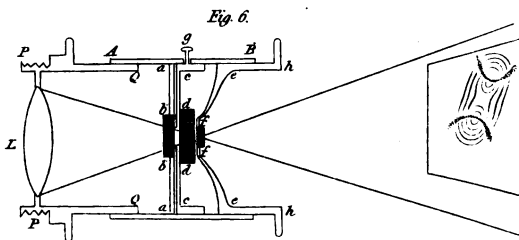
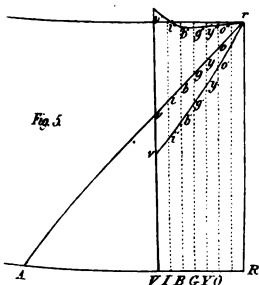
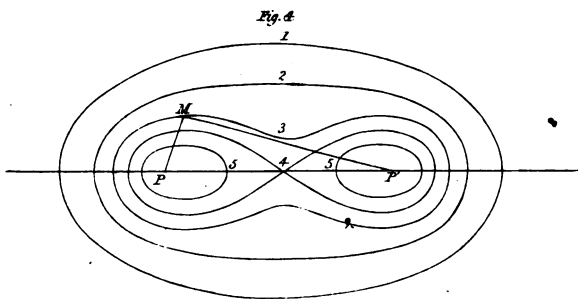
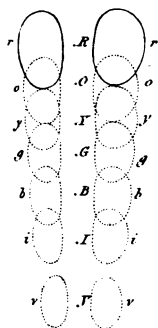


Fig. 3.



The substances, which I have examined most attentively, are sulphate of baryta, nitre, mica, and Rochelle salt, and the subjoined tables of tints developed for different inclinations in plates of the first and last of these, may serve as examples of the mode of action of the respective classes to which they belong on light, and will afford data for some calculations to follow. The first two columns contain the inclinations corresponding to similar tints of the incident ray on the moveable plate which carries the crystal, in the general apparatus imagined by M. Biot for observations of this kind. Were the plate cut in a direction precisely perpendicular to the optic axis (or line bisecting the angle between those of double refraction), and adjusted with perfect accuracy on the instrument, the excesses or defects of these angles above or below 90° would represent the angles of incidence. Neither of these conditions were, of course, exactly fulfilled. But it is obvious that the small errors in these particulars (which were ascertained not to exceed 1° or 2°) must affect the computed angles of refraction on both sides of the perpendicular with equal and opposite errors. The same may be said of any error arising from a slight prismaticity of the plate, which, however, must have been extremely small, the plate having always been rendered parallel by the delicate test of the sphærometer, within a very few divisions.* Consequently, in calculating on these data, the mean angle of refraction determined by the simultaneous use of both observations (their semi-difference being taken for the angle of incidence), may be expected to differ from the truth by an extremely minute quantity. The third column contains the tint developed in the ordinary pencil, and the fourth in the extraordinary. The last notices the remarkable points in the system of rings to which the tints and angles in the other columns correspond. The positions of the poles were determined by interposing a red glass between the crystal examined and the reflector used to polarise the incident light. The glass used for this purpose was of that kind occasionally found in old church windows, and whose manufacture seems to be numbered among arts now forgotten. It transmits almost the whole of the red rays, and part of the orange; while it completely stops all the more refrangible colours. I have endeavoured in vain to procure a specimen, whose limits of transmission are more confined. Such are said to exist, though very rare; and in the absence of such, the indications of that employed may be taken to correspond to the mean red rays.

* Each equal to the 23809th part of an inch.

TABLE I.—*Sulphate of Baryta. Thickness of Plate = 0.11964 in.*

Corresponding inclinations.	Ordinary pencil.	Extraordinary.	Remarks, &c.
49° 55' 194° 37'	Pink	Very pale blue green	Tints beyond the poles.
45 0 133 42	Bluish green	Pink	
46 0 132 45	Rich pink	Beautiful green	
132 0	Whitish	Dull purple	
46 58 131 37	Splendid green	Rich crimson	
131 0	Blue	Yellow	
47 53 130 45	Crimson	Fine green	
	Yellow, inclining to orange	Blue	
	Yellow	Purple	
48 51 129 30	Blue green	Rich crimson	
	Blue	Orange	Virtual poles p, p' , or points of coincidence
128 58	Purple	Pale yellow	
	Rich crimson red	Pale green or greenish white	
	Fine orange	Light blue	
	Pale orange yellow	Dark blue	
50 58 127 45	White	Sombre and very narrow purple	
	Bluish white	Scarlet or fiery red	
	Light blue	Orange	
	Sombre greenish blue	Ruddy white, or very pale orange	
52 0 126 50	Dirty and very sombre purple	White	
	Sombre and narrow pinkish red	White	The poles PP' Tints between the axes.
	Very pale violet or pinkish white	Greenish white	
	White	Dirty bluish green	
53 1 125 47	White	Narrow and very sombre violet purple	
125 30	White slightly yellowish	Violet	
125 0	Pale and dirty olive green	Violet white	
	Very narrow violet	White	
54 8 124 30	Blue, very sombre and narrow	White slightly yellowish	
	Light blue	Pale yellow	
	Bluish or greenish white	Indifferent purplish pink	
55 25 123 30	Yellowish white	Sombre and narrow purple	
	Light yellow	Dark blue	
	Dull orange pink	Pale greenish blue	
56 33 122 15	Sombre purple	Pale yellow green	
	Blue	Yellow	
57 30	Pale green	Fine pink verging to crimson	
57 50 121 0	Pale yellow	Purple	
58 15 120 15	Light yellowish pink	Greenish blue	
	Rich pink	Bluish green	
59 12 119 35	Pale purple	Greenish white	
	Blue green	Crimson	
60 45 118 20	White	Very pale purple	
117 40	Pink	Blue green	
62 0 117 5	Pale purple	Whitish	
62 58 116 25	Greenish blue	Pink	
63 25 115 35	White	Dull purple	
64 28 114 47	Pink	Bluish green	
65 40 113 20	Greenish blue	Pink	
67 30 111 30	Pink	Pale greenish blue	
69 15 110 0	Pale greenish blue	Pale pink	

In this plate, the virtual poles correspond very nearly to the second minimum of the extraordinary pencil beyond the poles P P'. The same plate was now reduced by grinding to the thickness 0.08816 inch. In this operation, care was taken to grind away the side of the plate most distant from the eye only, leaving the other perfectly untouched and unimpaired in its polish. The plate being reduced to exact parallelism by the spherometer was again examined, the same side still remaining next the eye. By this arrangement, the same angles of emergence from the posterior surface correspond rigorously to the same directions of the ray in the interior of the crystal, with respect to the axes of its molecules; and thus we avoid completely any errors which might arise from using plates cut at different angles, it being almost impossible to cut two plates precisely alike in this respect.

TABLE II.—*Sulphate of Baryta.* Thickness = 0.08816 in.

Corresponding inclinations.		Ordinary pencil.	Extraordinary.	Remarks, &c.
56° 0'	120° 0'	Pink, verging to orange yellow.	Blue, somewhat greenish ..	Poles for the mean red rays
		Yellow.	Dark blue	
57 30	120 47	Pale yellow.	Purple	
		Greenish white.	Pink.	
		Light blue.	Yellow, verging to orange	Tints beyond the poles.
		Dark blue.	Bright yellow	
55 57	122 15	Sombre purple.	Yellow white	
		Very indifferent sombre pink.	Bluish white	
		Pale yellow.	Dark indigo blue	Virtual poles
54 5	124 0	White.	Sombre violet	
		Very pale violet white.	Dusky greenish yellow	
52 57	125 33	Very sombre violet, almost black.	Pure brilliant white.	
		Sombre and dirty olive green.	White, rather ruddy	
		Very pale blue.	Orange white	
		White.	Sombre orange or brick red	
51 30	127 0	White.	Narrow purple	
		Orange white.	Blue	
		Bright orange.	Pale blue	
		Bright scarlet.	Bluish white	
		Narrow crimson.	White	
50 0	128 30	Purple.	White	
		Blue.	Yellow	
		Bluish white.	Rich crimson	
48 40	130 0	Yellowish or greenish white	Purple	
		Orange yellow.	Bright blue	
47 35	131 10	Rich crimson.	Green	
47 10	131 28	Purple.	Good yellow	
		Bright blue.	Pink yellow	
46 7	132 33	Good green.	Rich pink or crimson	
45 3	133 45	Rich pink.	Splendid green	
44 33	134 30	Greenish purple.	Pinkish white	
43 35	135 15	Good green, but pale.	Fine pink	
42 23	136 30	Pink red.	Greenish blue	
40 55	138 0	Pale bluish green.	Pale pink	
39 30	139 50	Pale pink.	Very pale greenish blue	

In this plate the virtual poles correspond to the second maximum of the extraordinary pencil. It is needless to detail the tints between the poles. The same plate once more reduced with the same precaution to leave the posterior surface untouched, developed the following series of colours beyond the poles.

TABLE III.—*Sulphate of Baryta.* Thickness = 0.05758 in.

Corresponding inclinations.		Ordinary pencil.	Extraordinary.	Remarks, &c.
60° 57'	122° 50'	Fine yellow	Indigo	Poles, for the mean red rays Tints beyond the poles
60 20	123 27	Pale yellow.	Purple	
		White, inclining to yellow.	Dull crimson red	
		Bluish white	Dull orange	
		Indigo	Yellow	
57 55	126 3	Sombre purple.	White	
57 50	126 7	Sombre reddish violet ...	White	
57 20	126 33	Dirty violet yellow.	White	
	127 30	Pale yellow.	Violet white	
55 40	128 2	White	Sombre violet	
55 33	128 20	Pure brilliant white	Black	Virtual poles
	128 30	White	Sombre dirty green	
		Pale orange.	Pale dirty bluish green	
53 40	130 10	Sombre orange or brick red	White	
53 27	130 30	Sombre and narrow purple	White	
		Blue	Ruddy white	
		Pale blue	Orange	
		Bluish white	Orange red	
	132 20	White	Narrow crimson	
51 3	132 40	Pale yellow, &c. &c.	Purple, &c. &c.	

Here the virtual poles p , p' correspond precisely to the first minimum of the extraordinary pencil.

In a plate of Rochelle salt, cut nearly, but not quite perpendicular to the optic axis, and whose thickness was 0.194425 in. the rings beyond the poles were almost entirely obliterated, while those between them exhibited the following singular succession of colours, which will show to what an extent the deviation from Newton's scale is carried in this substance.

TABLE IV.—*Rochelle Salt, perpendicular to the Optic Axis.*
Thickness = 0.194425 in.

Corresponding inclinations.	Ordinary pencil.	Extraordinary.	Remarks, &c.
201° 30'	330° 10'	White.	Poles PP' for red rays
207 10	Exceeding pale blue.	Exceeding pale pink	
209 0	Exceeding pale pink.	Exceeding pale blue	
210 30	Very pale blue.	Very pale pink	
212 30	Very pale pink.	Very pale blue	
213 20	Very pale blue.	Very pale pink	
215 25	Pale pink.	Pale blue green	
216 40	Pale blue.	Pale yellow pink	
218 0	Pink.	Blue green	
219 29	Pale blue.	Pale yellow pink	
220 58	Pink.	Greenish blue	
	White.	Very pale purple	
222 30	Blue.	Yellow pink	
223 45	309 30	Greenish blue	
224 50	308 15	Yellowish pink	
	Pale greenish blue.	Pale pinkish yellow.	
	Blue.	Pale greenish blue	
226 10	307 0	Dark blue	
	306 10	Pale purple	
	Pale pink.	Very pale violet pink	
227 25	Pale yellow.	Very pale yellow	
228 30	White.	White	
	Bluish or greenish white.	Pure white.	Virtual poles p, p'
229 3	304 10	A little violet	
	Exceeding pale yellow.	Very narrow dark blue	
229 50	Pale yellow.	Blue, sombre, and pale	
	Very pale tawny orange.	Pale yellow green	
231 20	302 20	White	
	Fine purplish crimson.	Fine crimson	
232 25	Very pale purple.	Pale purple	
	Very pale green.	Very pale green	
234 0	299 20	White	
	White.	Rich crimson	
234 40	Splendid crimson.	Pale purple	
	Pale purple.	Very pale green	
235 40	Very pale green.	White	
	White.	Rich crimson	
236 23	296 15	Pale purple	
	Rich crimson.	White	
238 50	294 35	Very pale green	
	Pale purple.	Good pink, almost crimson	
	Pale blue green.	Very pale purple	
240 12	292 55	Pale greenish blue	
	White.	White	
242 10	291 30	Pale yellowish pink	
243 40	289 50	Pale blue	
245 10	287 45	Very pale yellow	
246 50	286 45	Very pale lilac blue	
248 25	284 55	Yellow green	
	Pale lilac.	White	
	White.	Fine lilac	
250 40	282 30	White	
	Fine yellow green.	Fine yellow green	
252 57	280 12	White	
	Lilac.	White	
256 5	277 25	Fine lilac	
	White.	White	
259 20	274 40	Fine yellow green	
261 40	271 40	White	
266 40	266 40	Pale lilac.	The middle tint

In order, however, to avoid the effect of the dispersive power, which, at such considerable obliquities, would render the observations liable to some uncertainty, I cut another plate in such a manner that the perpendicular to its surface, instead of coinciding nearly with the optic axis, was directed very nearly to one of the virtual poles. Its thickness was then gradually reduced in the manner above described for sulphate of baryta, though, owing to the nature of the body, it was found impossible to avoid the necessity of re-polishing the posterior surface at each operation; but as this was done with all possible care, only a very slight error can have arisen from this cause.

TABLE V.—*Rockelle Salt.* Thickness = 0.11518 in.

Inclinations.	Ordinary pencil.	Extraordinary.	Remarks, &c.
277° 8'	Very pale pink.....	Very pale bluish green.....	Pole P for mean red rays Perpendicular incidence
261 8	
261 0	White, tinged with orange	Very fine intense indigo	
260 25	Yellowish or greenish white	Purple, rather sombre	
259 45	Very pale greenish blue ..	Indifferent purplish pink	
	Dull blue.....	Yellowish pink white	
238 50	Fine deep indigo	White inclining to orange	
258 30	Violet purple.....	Yellowish white	
257 35	White, a little tinged with violet	White, not very brilliant.....	The virtual pole p
	Yellowish white	Pale violet blue	
256 30	Pale yellow	Sombre indigo, inclining to violet, narrow, and well defined	
	Pale pink yellow.....	Sombre violet white	
255 10	Pinkish purple.....	Very pale greenish yellow	
254 30	Rich sombre purple, somewhat fiery	White, tinged with greenish yellow	
	Pale green	Fine crimson	
252 30	Extremely pale green ..	The richest deep damask crimson	
	White	Livid imperfect purple	
250 27	The richest damask crimson	Fine pale green	
	Livid imperfect purple....	White	
248 5	Pale bluish green	Fine rich crimson	
245 45	Pink, approaching to red..	Pale blue green	
243 30	Sky blue.....	Light pink, strongly inclining to orange red	
240 25	Pink orange	Pale greenish blue	
	Sky blue, inclining to lilac.	Fine yellow	
237 30	Pale bluish green	Fine pink, a little purple	
237 0	Splendid yellow green ...	Lilac	
234 20	Rich lilac	Splendid green	
230 10	Splendid green	Rich lilac	
	White.....	White	
225 45	Lilac blue.....	Splendid yellow green	
	White.....	White	
220 40	Splendid green yellow ...	Pale lilac blue	
	White.....	White	
211 40	Pale lilac.....	Pale greenish yellow	
199° 30'	Fine yellow green.....	Fine lilac	

Here the virtual pole was coincident with the fifth maximum (or thereabouts) of the extraordinary ray from the pole P; the succession of tints, however, unless close to the virtual pole, is omitted, in order to shorten the table.

TABLE VI.—*Rochelle Salt.* Thickness = 0.08557 in.

Inclina- tions.	Ordinary pencil.	Extraordinary.	Remarks, &c.
208° 0'	Good light pink.	Light blue green	Pole P for mean red Perpendicu- lar incidence
278 0	
278 20	Very pale yellow green....	Bluish purple	
278 25	Very pale yellow green....	Violet	
280 0	Very pale bluish green...	Very light pink, or pinkish white	Virtual pole
	Indigo.....	Very pale yellow, almost white	
	Pale violet.....	Very pale yellow	
281 30	White.....	White, perfectly equal and alike...	
	Very pale greenish yellow	Pale lilac	
282 35	Pale greenish yellow	Sombre lilac purple	
	Pale pinkish yellow	Dull and impure blue	
	Pale pink.....	Pale yellow green	
285 15	Crimson	Yellow green	
285 45	Rich fiery damask crimson	Very pale yellow green	
	Livid imperfect purple....	Very pale pink yellow	
287 50	Fine light green.....	Fine pink	
288 45	Very pale green.....	Splendid crimson	
	Good crimson.....	Blue green	
291 40	Crimson, almost scarlet, &c.	Pale blue green, &c.	

In this plate the virtual pole fell about half way between the fourth maximum and the fifth minimum of the extraordinary ray from the apparent pole P for the mean red rays. When once more ground down, it gave as follows :

TABLE VII.—*Rochelle Salt.* Thickness = 0.05437 in.

Inclina- tions.	Ordinary pencil.	Extraordinary.	Remarks, &c.
262° 25'	Fine pink.....	Fine light blue green.....	Pole P for mean red rays
	Indifferent purple	Yellowish white	
271 20	Indifferent lilac pink	Very pale greenish yellow	Perpendicu- lar incidence
278 25	Pale yellow, inclining to orange.....	Blue, rather pale.....	
	Fine pale yellow	Beautiful sobre indigo	
280 30	Yellowish white, or pale yellow.....	Violet	
282 0	White.....	White, with an almost imperceptible tinge between yellow and violet..	Virtual pole
	Very pale blue	Yellow white	
284 50	Sombre indigo.....	Pale yellow white	
	Very pale blue.....	Extremely pale pink white	
	Yellow green.....	Lilac pink	
	Pale yellow green	Deep lilac pink	
286 25	Greenish white.....	Rich, but sombre purplish crimson	
	White.....	Dull purple	
	Very pale pink.....	Good blue green	
288 33	Deep fiery crimson	White	
	Very dull purple (greenish)	White	
	Blue	Pink yellow	
294 50	Very pale blue, &c.	Rich orange, bordering on red, &c.	

III. On the Causes of these Phenomena.

The developement of colour along the axis of double refraction is at first sight analogous to the production of the secondary tints along the axis of rock crystal, discovered by M. Arago, and recently explained by M. Biot, in a masterly memoir communicated to the Academy of Sciences, on the hypothesis of a force inherent in its molecules independent of their state of aggregation, by which they communicate a rotation in an invariable direction to the axes of polarisation of the luminous rays. And this analogy is partially supported by the fact, that the tint developed along the axis descends in the scale of colour as the thickness increases.

A more scrupulous examination, however, will show, that its origin must not be sought in any cause of this nature, for (not to mention the impossibility of explaining the phenomena of the virtual poles by this hypothesis) if we place the principal section of the crystal in the azimuth zero, the extraordinary image will be found to vanish completely for every angle of incidence, and whatever be the thickness of the plate. I may add too that I have in my possession a crystal of quartz, which exhibits with tolerable distinctness in some parts the phenomena of two axes, and the appearances produced by the interference of the secondary tints in this specimen; while they agree completely with M. Biot's explanation, differ entirely from those which form the subject of this paper.

Neither are the phenomena above described explicable on any supposition of a peculiar action of the crystal on the differently coloured rays, analogous to its ordinary or extraordinary dispersive power, by which the periods of alternate polarisation of the molecules of some colours, should be lengthened, and of others contracted, so as to disturb that exact proportionality to their periods of easy reflection and transmission, which M. Biot has proved to be a necessary condition for the production of the tints of Newton's scale. It is true, such laws of action may be imagined, and I shall presently show must really exist; in all crystals probably to a small extent, but in two instances at least, to a surprising degree. But this alone will avail us nothing. To show this, and at the same time obtain a general analytical expression for the tint developed at any inclination, and for every hypothesis of the action of the crystal on the differently coloured molecules, let us denote by c the length of a complete period of easy transmission and reflection, or the extent of one pulse, on the undulatory hypothesis in vacuo, and at a perpendicular incidence for any homogeneous ray, and let C denote its colour and proportional intensity, or illuminating power, in the prismatic spectrum. Then will the formula representing a beam of white light intromitted into the crystal, be

$$C + C' + C'' + \&c.$$

from one end of the spectrum to the other.

Let n be the number of periods (each consisting of a double alternation) and parts of a period performed by the elementary pencil C , in its passage through the medium: then, according to the theory of M. Biot, when n is 0, 1, 2, 3, &c. ad inf. the pencil will wholly pass into the ordinary image; but when the values of n are $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, &c. it will wholly * be thrown into the extraordinary one, and in the intermediate states of n , partly into one, and partly into the other. These conditions are satisfied if we represent by $\sin.^2(n\pi)$ the intensity of the ray in the ordinary image, taking unity for its original intensity; and it will, I believe, be found, that the gradation of intensity given by this formula for the intermediate values of n , will agree sufficiently with the judgment of the eye to warrant its adoption.† The part of the elementary pencil C then, which enters into the extraordinary image, will be $C \cdot \sin.^2(n\pi)$. Let us denote by $S \{C \cdot \sin.^2(n\pi)\}$ the aggregate of all such elements from one extremity of the spectrum to the other, or take

$$S\{C \cdot \sin.^2(n\pi)\} = C \cdot \sin.^2(n\pi) + C' \cdot \sin.^2(n'\pi) + \&c.$$

Then will this expression represent the tint developed in the extraordinary image, and, consequently, $S \{C \cdot \cos.^2(n\pi)\}$ that in the ordinary one.

Now, n , the number of periods performed depends, first, on the nature of the ray, or on c ; secondly, on the intrinsic energy of the action of the medium on that ray; and thirdly, on the direction of its course, the thickness of the plate, and whatever other cause or limit of periodicity may happen to prevail. Hence we may take $n = M \times k$, k being a function of c , dependent only on the nature of the body through which the ray C passes, and M being a certain multiplier whose form we shall consider presently. This substitution made, the expression for the tint becomes $S \{C \cdot \sin.^2(Mk\pi)\}$

In the theory of the Newtonian colours of thin plates and the polarised rings in crystals with one axis, the multiplier M is independent on c , varying only with the direction of the ray and the thickness of the plate. It is, therefore, the same for all the coloured rays, and the tint, for any value of M , will be

* The amplitude, or total extent, of each oscillation of the plane of polarisation is here supposed 90° , in which case the contrast of colour in the two pencils is at its maximum. This is the case in the situation we are considering, but in general the intensity of the extraordinary ray, instead of being represented by $\sin.^2 n\pi$, will have an additional factor, a function of the azimuth A of the principal section of the crystallized plate and the position of the refracted ray, and which becomes unity when $A = 45^\circ$, and the plane of incidence is that of the principal section. It is on this factor that the gradation of brightness in the isochromatic lines, and the black cross or hyperbolic branches which intersect them, depend. But it is not my intention at present to enter on this part of the subject, for reasons to be explained further on.

† No part of our subsequent reasoning depends on the form of this function. It is sufficient to know that it must be a periodical, and even function of n . It is only in the computation of numerical values that it is necessary to make any more precise assumption.

$$C. \sin.^2 (M k \pi) + C'. \sin.^2 (M k' \pi) + \&c. \quad (a)$$

Now, suppose M to begin from zero, and to pass, by a variation either in the direction of the ray or thickness of the medium, or both, through all gradations of value, to infinity, or to its maximum, if not susceptible of infinite increase: then we see that for every value of M a certain peculiar tint will arise, and that, provided M commence at zero and continue increasing, the same succession of tints will invariably be developed in the same order. Consequently, if we fix upon any two tints in this scale of colour, or any two values of M , the same succession and the same number of alternations of colour must invariably intervene between them, however we pass from one to the other.

In a crystal with two or more axes, the value of M for any ray C must of course be zero in the direction of the axis, and, therefore, if the same supposition of the independence of M on c be made, the same conclusions should follow; namely, first, that the extraordinary ray must always vanish in the pole, whatever be the thickness of the plate; and, secondly, that the same succession and number of alternations of colour should intervene between the pole and any assigned unequivocal tint, such as black, or the pure brilliant green of the third order of Newton's scale. Both these conclusions are totally at variance with the facts above detailed, as to the developement of colour in the poles, and the situation in the order of the rings of what we have called the virtual poles. Hence we are necessitated either to give up the theory of alternate polarisation altogether, or to admit the dependence of the multiplier M on c , or on the nature of the ray. Let us see to what this will lead us.

According to the theory of the polarised rings, if extended to crystals with two axes, the number of periods performed in a given space ($= 1$) by a molecule of a given colour, transmitted in a direction making angles θ, θ' , with the axes, can only be a function of the form $k, \psi(\theta, \theta')$, k depending on the intensity of the polarising force; or, as before, being a function of c , the nature of the ray, and of the intrinsic energy of the molecules of the crystal. Now if we call t the thickness of the plate, and ϕ the angle of refraction, $\frac{t}{\cos. \phi}$ is the length of the path described, and, therefore, we must have for the number of periods

$$n = \frac{k t}{\cos. \phi} \cdot \psi(\theta, \theta');$$

so that the value of M must be $\frac{t \cdot \psi(\theta, \theta')}{\cos. \phi}$, which must be a function of c . Now t is obviously independent of it; and if we neglect at present the very trifling effect at moderate incidences of the ordinary dispersive powers of the media examined,* ϕ is also.

* It is easy to see that in the two classes of crystals above described, the effects of the dispersive powers will be opposite to each other, in one opposing, and in the other

It is, therefore, in the form of the function $\psi(\theta, \theta')$ that we must look for the cause of the phenomena; and since, we have $\theta' = \theta + 2a$, $2a$ being the angle between the axes (because the observations are made in the principal section) we see that $\psi(\theta, \theta + 2a)$ must involve c , and consequently, θ being arbitrary and independent, a must be a function of c . In order then to render the theory of alternations applicable, we must admit the angle between the axes of double refraction to differ in the same crystal for the differently coloured rays. We must now show that this supposition is sufficient to represent the phenomena correctly.

The symmetry of the rings and total evanescence of colour in the principal section at an azimuth zero, requires that the axes of all the different colours shall be symmetrically arranged, on either side of a fixed line (which may be called the optic axis), in this plane, or in one perpendicular to it. At present we need only consider the former case. Let a represent the angular distance of the axis for any one standard species of ray C (the extreme red, for instance) from this line, $a + \delta a$, the same distance for any other ray. Then the distance of the transmitted ray C , from the axes of rays of that colour being θ, θ' , the corresponding distances from *their* respective axes for rays of any other colour C' emerging in the same direction will be $\theta - \delta a + \delta \phi$ and $\theta' + \delta a + \delta \phi$, $\delta \phi$ being the difference ($= \phi' - \phi$) of the angles of refraction, corresponding to the same incidence, for the colours C, C' . The positive values of θ here reckon outwards from the pole; δa is negative for crystals of the second class, and $\delta \phi$ is negative or positive according as C or C' is the less refrangible colour.

Let us for a moment consider rays of only these two colours. The portion of the extraordinary pencil due to them will be

$$C. \sin.^2 \left(\frac{k t}{\cos. \phi} \psi(\theta, \theta') \cdot \pi \right) + C'. \sin.^2 \left(\frac{k' t}{\cos. \phi'} \psi(\theta - \delta a + \delta \phi, \theta' + \delta a + \delta \phi) \cdot \pi \right).$$

The rays of these colours of the same order in their respective series of rings, will, therefore, coincide, and that in the proper degree of proportional intensity for the production of a white image, provided we suppose

$$\frac{k}{\cos. \phi} \cdot \psi(\theta, \theta') = \frac{k'}{\cos. \phi'} \cdot \psi(\theta - \delta a + \delta \phi, \theta' + \delta a + \delta \phi); \quad (b)$$

which, since $k, k', a, \delta a$, are constant elements, ϕ, ϕ' determinate functions of θ , and $\theta' = \theta + 2a$, suffices to determine δ .

If we suppose C and C' to represent the extreme red and violet rays, it is evident that the coincidence of the extraordi-

conspiring with the causes which produce the deviation of tints. In the tables, Nos. V, VI, VII, where the virtual poles were observed almost at a perpendicular incidence, the influence of the dispersive power is quite insensible.

nary pencils of the same order for these two extremes, will ensure that of the intermediate ones, at least very nearly. It would do so precisely, were the value of δa for any intermediate ray, such a function of k as would result from making θ constant in the preceding equation, because the two laws, that of the dispersion of the axes, and that of the magnitude of the rings of different colours, would then act in exact opposition to each other throughout their whole extent. It is in fact a case precisely analogous to that of the compound achromatic prism, where, if the law of dispersion in the one medium were identical with that in the other, a perfectly colourless pencil would emerge, and when these laws differ, the coincidence of the red and violet rays ensures an approximate coincidence of all the rest. Should these laws, however, differ very considerably, an uncorrected colour will appear at the point so determined, and a nearer approximation will be obtained by uniting two of the more powerful intermediate rays, such as, for instance, the mean red and the blue, or limit of the green and blue.

This then is the origin of the virtual poles or points beyond or between the axes where the tint rises to a white of the first order, more or less feeble, or even to an absolute black; and we may now see the reason why the tints, in reckoning from these points, approximate in a general way to the Newtonian scale. In fact, the periods of the more refrangible rays being performed more rapidly than those of the less, if we suppose the coincidence above spoken of to take place at any point (the minimum for instance) of the n th ring, the intervals between the n th and $(n + 1)$ th minimum will be greatest for the red, and least for the violet, &c. Consequently, when the violet next disappears totally from the extraordinary pencil, there will remain yet a little of the red, less of the orange, and so on, and this difference increasing at every succeeding minimum on either side, will produce a succession of colours approximating in a general way to Newton's scale. This approximation will, however, be much less close on the side of the virtual pole towards the nearest axis, because the disturbing influence of the separation of the axes on the figure of the rings and the law of their successive intervals, is much more sensible than at a distance from the pole. This will be evident if we consider that in the interval between the extreme coloured axes, the tints will be regulated entirely by the law of their distribution. Now this is perfectly corroborated by the succession of tints in the foregoing tables, as well as by numerous experiments made on other bodies.

(To be continued.)

ARTICLE X.

Results of " Meteorological Register kept at New Malton, Yorkshire, in the Year 1820. By Mr. J. Stockton.

1820.	BAROMETER.					SIX'S THERMOM.				WINDS.										WEATHER.			RAIN.			
	Mean.	Max.	Min.	Range.	Spaces described in inches, &c.	Number of changes.	Mean.	Max.	Min.	Range.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Var.	Brisk.	Boisterous.	Wet.		Snowy.	Hail.	Quantity in inches, &c.
Months.																										
Jan.	29.645	30.90	28.24	2.66	10.74	16	30.500	48	7	41	3	3	2	1	5	8	6	1	1	2	2	2	4	9	0	2.10
Feb.	29.756	30.25	29.10	1.15	5.97	14	37.120	54	25	29	2	5	3	1	13	3	1	1	0	2	2	6	1	0	0	2.14
March.	29.668	30.40	28.35	2.05	8.07	10	39.840	58	14	44	7	3	1	1	2	7	4	5	1	1	3	6	6	6	0	2.87
April.	29.700	30.60	28.94	1.66	5.95	14	48.185	68	30	38	1	5	3	3	0	4	7	3	4	1	0	8	0	3	3	2.18
May.	29.466	30.21	28.70	1.51	6.10	13	52.984	74	30	44	1	4	2	2	6	13	1	0	2	2	0	1	18	0	1	4.25
June.	29.704	30.27	28.94	1.33	5.18	12	57.366	85	40	45	5	3	0	0	0	6	5	6	5	5	0	0	12	0	1	2.24
July.	29.703	30.05	29.04	1.01	3.37	9	59.612	78	46	32	9	6	3	0	0	3	2	6	2	1	0	9	0	0	0	2.56
August.	29.555	30.04	29.02	1.02	6.21	13	54.258	77	42	35	2	2	0	0	6	14	3	3	1	3	1	19	0	1	1	3.40
Sept.	29.753	30.18	28.98	1.20	6.65	19	53.200	69	35	33	2	1	0	2	6	6	6	5	2	3	1	8	0	1	1	1.63
October.	29.315	30.58	28.05	2.53	11.43	19	46.000	62	32	30	2	7	0	3	8	2	2	0	1	2	5	13	0	0	0	2.20
Nov.	29.702	30.22	29.12	1.10	6.21	14	39.833	53	28	25	4	5	5	1	6	9	0	0	0	3	1	9	1	0	0	2.05
Dec.	29.800	30.20	29.35	0.85	5.27	16	39.903	55	29	26	1	8	7	0	2	9	2	1	1	1	7	2	6	3	0	1.91
Annual means, &c.	29.647	30.90	28.05	2.85	80.95	169	46.900	85	7	78	39	52	26	14	54	84	39	37	21	32	16	118	20	7		29.43

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Jan. 9. Wind, N.E.	30·900
Lowest ditto. Oct. 17. Wind, S.	28·050
Range of the mercury.	2·850
Mean annual barometrical pressure	29·647
Greatest range of the mercury in Jan. (in <i>nine</i> days) ..	2·660
Least ditto, ditto, in December.	0·850
Mean annual range of ditto	1·505
Spaces described by ditto.	80·950
Total number of changes in the year	169·000

Six's Thermometer.

Greatest observations, June 26 and 27. Wind N. and var.	85·000°
Least ditto, Jan. 1. Wind, W.	7·000
Range of the mercury in the thermometer	78·000
Mean annual temperature	46·900
Greatest range in June	45·000
Least ditto in November	25·000
Mean annual ditto	34·333

Winds.

	Days.
North and East	65·000
North-east and South-east.	66·000
South and West	93·000
South-west and North-west.	121·000
Variable	21·000

Rain, &c.

	Inches.
Greatest quantity in May	4·250
Least ditto in September	1·630
Total amount for the year	29·430

ARTICLE XI.

Observations on Mr. Perkins's Account of the Compressibility of Water. . By P. M. Roget, MD. FRS. &c.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Bernard-street, Russell-square, Jan. 25, 1821.

ON reading the account which Mr. Perkins has lately given in the *Philosophical Transactions*,* of his very interesting experiments on the compressibility of water, from which he has deduced a result differing widely from that of Mr. Canton; and being desirous of ascertaining the exact quantity by which they differed, I was induced to calculate the degree of compression which was produced by the piezometer in the first experiment, from the data furnished by Mr. Perkins, and was thus led to the discovery of a very material error which he has committed in his computation. He states the compression affected by a pressure of 100 atmospheres to be "about one per cent;" whereas the real amount of the compression was, in fact, only $\frac{1}{212.36}$, or a little less than *one-half per cent.* as may easily be verified by any one who will be at the trouble of going through the calculation. It is remarkable that this amended result agrees very nearly with that of Canton. This will be best evinced by computing the heights of the modulus of elasticity of water, in the two cases, according to Dr. Young's method. As deduced from Canton's experiments, the height of the modulus is 750,000 feet;† while those of Mr. Perkins, when correctly computed, would show it to be 743,260 feet—the difference being less than one-hundredth of the whole. So near an agreement, in experiments conducted by different methods, is very satisfactory, and bears the stronger testimony in favour of the accuracy of those of Mr. Perkins, in as much as he was himself not aware of that agreement. It is much to be wished that this gentleman, to whom science and the arts are already much indebted, will persevere in the prosecution of his interesting inquiry, by the aid of the ingenious apparatus he has invented, and from which the discovery of many curious and important facts may be expected.

I am, dear Sir, most truly yours,

P. M. ROGET.

* For 1820, Part II. p. 324.

† Young's *Lectures on Natural Philosophy*, i. 376.

ARTICLE XII.

Description of a new Substance found in Ironstone.

By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

Bath Easton, Jan. 17, 1821.

DURING a visit to South Wales in the course of last summer, a substance, found in the ironstone of Mezthyr Tydfil, was put into my hands for examination. It appears to me to differ from all the varieties of bituminous matter hitherto discovered, sufficiently to form a separate species. Should my opinion be correct, I would propose that it should be distinguished by the name of *Hatchetine*, in reference to the eminent chemist, to whom we are indebted for the most valuable contributions towards the history and analysis of this class of natural substances. I remain, Sir,

With much esteem, yours sincerely,

J. J. CONYBEARE.

The colour varies from yellowish-white to wax and greenish-yellow.

The texture is sometimes flaky (like that of spermaceti), sometimes subgranular, like that of bees' wax.

The lustre is, in the flaky variety, slightly glistening and pearly; in the other dull.

The transparency is in the flaky (especially in thin laminæ) considerable: other specimens are opaque.

It is very soft, not harder than soft tallow.

It has no elasticity, and no odour.

It is very fusible; melts when placed in warm water under 170°.

It is very light.

The only analogous substances to which *Hatchetine* might be compared are petroleum and elastic bitumen. From the former, it differs in its consolidation; from both, in the greater part of its external character, and in its want of smell. It melts under 170°, whereas thin bitumen does not melt even in boiling water. Like elastic bitumen, it is readily soluble in ether; and each solution, by spontaneous evaporation, leaves a viscid oily matter in separate drops, but that from *Hatchetine* is still inodorous; while that from elastic bitumen retains strongly the peculiar smell of that substance. *Hatchetine* distilled over the naked flame of a spirit-lamp assumes the bituminous smell, and gives over a butyraceous substance of a greenish-yellow, coaly matter

remaining in the retort* ; at a lower heat, it gives over a light oil. I have no means of ascertaining its ultimate constituents ; but have little doubt that the above characters are sufficient to authorize our considering it as distinct from petroleum, asphalt, and elastic bitumen, the only three species (with the exception of retinasphalt, which occurs under very different geognostic relations, and appears to be the result of a very different natural process) into which substances of this class have hitherto been divided by mineralogists.

The Hatchetine is found filling small contemporaneous veins lined with calcareous spar and small rock crystals (termed the Mezthyr diamonds) in the ironstone.

ARTICLE XIII.

Electro-magnetic Experiments.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Cambridge, Jan. 22, 1821.

IF, as I imagine, the following electro-magnetic experiments are new, you will oblige me by inserting them in the next number of the *Annals of Philosophy*.

A weight was suspended from a small horse-shoe magnet ; on connecting the north pole of the magnet with the copper side of a pair of galvanic plates, the weight was attracted more strongly ; on reversing the wires, it fell.

A small magnetic bar being placed in the galvanic circuit, its south pole being in connexion with the positive end of the battery, the magnetism was destroyed in half a minute.

A connecting wire, 1-15th of an inch diameter, being placed horizontally in the plane of the magnetic meridian, over a compass, the deviation of the needle was between 80° and 90° ; in Oersted's experiments, it is stated to be about 45° .

On using a smaller wire, the deviation was diminished ; and when the diameter of the wire was 1-200th inch, it was less than 20° .

This diminution of the deviation took place whether the small wire were immediately over the compass, or interposed in any other part of the circuit.

When the connexion was made at the same time by the two above-mentioned wires, the smaller being three inches long, the longer five feet, the needle of a compass placed under the smaller deviated about 10° ; that of another under the larger deviated

* Elastic bitumen, at the same heat, gives over a yellowish oil perfectly fluid.

80°. On removing the larger wire, the deviation under the smaller increased, &c.

When the circuit was made by a glass tube filled with mercury, the deviation was the same as with a wire of the same thickness; but on removing the mercury; and coating the tube with silver leaf, the deviation was not more than between 5° and 10°.

The electro-magnetic influence was not diminished by raising the connecting wire to a red heat; but on interrupting the circuit by water, it was destroyed, though the connecting wire placed immediately over the compass decomposed water at each end.

If the connecting wire were bent alternately from N. to S. and from S. to N. the magnetic needle deviated to the W. of the N. in all the former, and to the E. of N. in all the latter bendings, whatever were their number, and in whatever part of the wire they were placed.

When the connecting wire was made to pass from N. to S. over the needle, and from S. to N. under it, the electro-magnetic influence was doubled.

A single pair of rather large plates was found, as in Oersted's experiments, to be more efficacious than a number of plates with the usual arrangement; but when wires from all the zinc plates were connected on one side, and from all the copper plates, on the other, the electro-magnetic influence increased with the number of plates.

It was found that the relative quantities of galvanism generated in different experiments might be estimated with great readiness and accuracy by using a graduated slide carrying that connecting wire over a fixed compass with a standard deviation.

I am, Sir, respectfully yours,

J. C.

ARTICLE XIV.

ANALYSES OF BOOKS.

Untersuchungen über den Magnetismus der Erde. Von Christopher Hansteen, Professor der Angewandten Mathematik an den Norwegischen Universität. Christiania, 1819.

Researches on the Magnetism of the Earth. By Professor Hansteen, of Christiania. 1 vol. 4to. 650 pages.

I HAVE just received a copy of the first volume of this celebrated work, which I have been very anxious to see. I am unwilling to peruse it till I recover the Atlas, which ought to have accompanied it, and which has been unfortunately lost on the way between Christiania and London; but I cannot avoid

immediately announcing it to the public, and pointing out the importance of publishing it in an English translation. I shall take a future opportunity of giving a detailed account of it to the readers of the *Annals of Philosophy*. At present I shall merely give the titles of the great divisions of the work, and add some important facts and corrections which Professor Hansteen has pointed out to me in a letter, dated Christiania, Nov. 21, 1820.

After an introduction of 14 pages, Prof. Hansteen treats of,
 1. Halley's lines and of their motion between 1600 and 1800.
 2. Of the lines of inclination, and of the magnetic intensity.
 3. The number, position, and periodical gyration of the magnetic poles round the pole of the earth. 5. Calculation of Halley's lines from the first imperfect theory of Euler. 5. Mathematical theory of magnetism. 6. Application of the theory of magnetism to the theory of the magnetical declination, inclination, and intensity, in a given place, whose geographical position is known. 7. A more accurate determination of the position of the magnetical axes, their size and relative intensities. 8. Of the daily variation of the needle. The whole terminates with very ample tables of the declination and dip in various parts of the earth.

Prof. Hansteen requests me to inform the public that his seventh chart, showing the dip of the magnetic needle, is erroneous, because, when projecting it, he was unacquainted with the original observations made in the course of a voyage to the Northern Pacific Ocean by Capt. Cook and Mr. William Bayley. Led astray by some observations of Krusenstern, he was induced in the corrections and additions to his work, p. 21, 22, to contradict M. Biot, who insists, in his *Traité de Physique*, tom. iii. p. 131, that the line of no dip (magnetic equator) cuts the equator of the earth three or four times. "But," continues Prof. Hansteen, "Biot is in the right, and the accompanying corrected chart shows the dipping lines in the Pacific in their right form. Your mentioning in your *Annals*, in a few words, that I myself acknowledge my error, will be conferring a particular obligation on me."

"On observing the oscillations of a magnetical steel cylinder, suspended by a silk-worm thread in a small box with glass openings, I have made the curious discovery that the magnetical intensity of the earth has a daily and annual variation. By a chronometer of Arnold's, I observe five times every day, at stated hours, the time required for completing 300 vibrations; and these observations I have already continued for nearly a year. From early in the morning, the intensity is on the decrease until between 10 and 11 o'clock in the forenoon, when it has reached its minimum. From this time it increases, at first slowly, afterwards more rapidly, till it reaches the maximum at four o'clock afternoon in the winter, and between six and eight in the summer. At times it does not reach this maximum

till about ten in the evening. In the winter, the intensity is much stronger than in summer. The greatest intensity appears to happen in the month of January; the least happened this year (1820) on the 13th of July. The daily variations are much greater in summer than in winter.

“Great irregularities occur at times, especially on those days, when the moon passes the equator, or on the quarter days of the moon. Similar great changes I have observed during the equinoxes. The influence of the *northern lights*, as already observed by Mr. Humboldt, is very remarkable, and frequently it does not regain its former strength till after the lapse of 24 hours.”

“I have likewise found in the same manner that every perpendicular object, of whatever materials; for instance, a tree, the wall of a house, &c. has a magnetic north pole at the foot, and a south pole at the top.”—T. T.

*An Essay on Chemical Analysis, chiefly translated from the Fourth Volume of the last Edition of the *Traité de Chimie Élémentaire* of L. J. Thenard, with Additions, comprehending the latest Discoveries and Improvements in this Branch of the Science. With Plates. By John George Children, FRSL. and E. FAS. &c. &c.*

Chemical analysis, owing to the great number of simple substances with which the science has been enriched, and the consequent increase of compounds, is now rendered a subject of considerable difficulty and complexity. These have, however, in many instances, been fortunately diminished by the discovery of the doctrine of definite proportions, which the chemist may now avail himself of in proof of the justice of his views, and the correctness of his analyses. It must, however, at the same time be admitted, that this discovery, vast as its importance is, is liable to be, and probably has been, misused by some who have determined what the composition of a body ought to be, rather by a comparison of numbers than by the slow and tedious process of analysis. It is, however, to be remarked that the doctrine of definite proportions, although it may be called in to prove the correctness of an analysis, ought never to supply the place of one—I do not mean that it is not allowable to form conjectures as to the composition of bodies by the aid of the atomic theory; I only mean that these conjectures should not be stated as facts to be absolutely relied on.

Mr. Children has stated so clearly the motive which induced him to undertake the present work, that I shall give his own words on the occasion: “On reading the fourth volume of M. Thenard’s *Traité de Chimie Élémentaire, Théorique et Pratique*, which treats exclusively of chemical analysis, and in a manner much more satisfactory and complete than any other

work I had before met with, it struck me that if translated into our language, it could not fail to be of great utility to the English chemist." Mr. Children then observes, that to the first edition of the author he had added the valuable matter contained in the second edition, which appeared while the translation was proceeding.

Mr. Children was, I think, judicious in the choice of the author whom he selected, for giving to the English chemist a more full account of the minutiae of analysis, than had ever appeared in our own language.

Those parts of the work which depended merely upon the translator, it is hardly requisite to state have been accurately performed by Mr. Children; but it would be doing him great injustice not to mention, that much new and valuable information, not to be found in the original work, has been added: in saying this, I do not mean merely that such notices have been collected as are within the reach of every one who inspects the various sources of chemical knowledge; but on several occasions Mr. Children has given us the results of his own experiments and observations, and which, I think, cannot fail to be useful. The directions for the use of the blow-pipe, and the appearance which certain substances present after its action, form a very excellent part of the appendix; and Mr. Children shows, by the attention which he has paid to the subject, that he has duly appreciated the value of Bergman's remarks upon the blow-pipe, with which he very aptly concludes his remarks on this useful instrument.

On the subjects of the analyses of vegetable and animal substances, a concise but clear account of the proximate principles of vegetable bodies has been introduced. It is, however, to be observed, that by an oversight, the very widely diffused and valuable vegetable product tannin has been totally omitted, nor is its combination with gelatine and albumen mentioned.

On the subject of the atomic theory, Mr. Children justly remarks, "that in the great progress which chemistry has made within a few years, one of its most important steps towards perfection as a science, is the establishment of the atomic theory." A knowledge of the principles upon which it is founded, affording, he observes, to the practical analyst an easy and almost infallible test of the accuracy of his experiments.

Mr. Children then gives a sketch of the atomic theory, which will afford the young chemist much useful information on this curious and highly interesting topic. It may be, perhaps, out of place here, but I cannot help remarking, the difference which exists among chemical philosophers as to the numbers by which they represent hydrogen and oxygen; for it will be found that they differ considerably both in quantity and proportion. Thus Mr. Dalton represents hydrogen by 1; and oxygen by 7. Sir H. Davy, hydrogen, 2, and oxygen, 15. Dr. Henry, hydrogen, 1, and oxygen, 7.5. Mr. Brande agrees with Dr. Henry

Dr. Wollaston represents hydrogen by 1.32, and oxygen by 10. Dr. Thomson, hydrogen, 0.125, and oxygen, 1. It is much to be regretted that these variations should exist: they produce the ill effect of appearing to render a subject difficult which is easy of explanation, by merely stating, as Mr. Children has done, that whatever portion we take of a compound mass, "it must contain a certain number of atoms of each substance; and although we know nothing of their actual number, still we obtain by analysis the proportion that the atoms of one kind bears to those of the other; or, supposing the compound to contain an equal number of each, the proportionate weights of the atoms themselves."

On the subject of the analysis of mineral waters, Mr. Children has added much to the original work of M. Thenard. He has particularly noticed the opinions of the late Dr. Murray, to the ingenuity of whose views on this subject, as well as on every other, I wish (however useless) to add my assent. I confess, however, for reasons which I shall take another opportunity of stating, that I do not accede to the correctness of his views when supposing that those salts which exist in a mineral water are such as result from the union of those acids and bases as form the most soluble compounds. Whatever I may advance on this subject, I am nevertheless of opinion that the results obtained by Dr. Murray in his analysis of sea water, show that the mode which he employed was generally correct; and Mr. Children has judiciously inserted it.

In the Appendix, Mr. Children has given Dr. Marcet's method of detecting the presence of arsenic, observing "that the unhappy frequency with which arsenic has been employed for the most nefarious purposes, renders an infallible mode of detecting its presence, *when in very minute quantity*, a great desideratum in medical jurisprudence." "It has been objected," says Mr. Children, "to this test, that if a phosphate be present, its indications are ambiguous; for the colour of phosphate of silver is not much unlike that of arsenite of silver. An experienced eye, however, will readily distinguish between them; the latter being of a brighter yellow than the former." I must confess that this ambiguity is to me an insuperable objection to what is termed the silver test. I have seen precipitates occasioned by the phosphoric and arsenious acids so similar in colour that I could not distinguish any difference, and much less any variation which would be a sufficient guide for deciding on the solemn and unhappy occasions in which evidence is required. It is, however, but proper to add, that Dr. Marcet no longer depends upon the evidence afforded by colour of the precipitate of arsenite of silver, unless it be corroborated by other appearances.

"The best method that I know of," says Mr. Children, "is to pass a current of sulphuretted hydrogen gas into a suspected

solution, when, if arsenious acid be present, it will occasion the appearance of a fine lemon-yellow colour through the liquid, but will have no action on phosphate of silver. That substance, however, may prevent the yellow colour from appearing, although arsenious acid be actually contained in the solution; but the addition of a very few drops of very dilute pure nitric acid will immediately produce it. If, therefore, both the silver test and the sulphuretted hydrogen concur in indicating the presence of the poison, no reasonable doubt can be entertained respecting it; but it is certainly an additional satisfaction to reduce a portion to the metallic state, or at least to sublime the oxide so as to render its peculiar albaceous odour distinctly evident, where enough can be procured for the purpose, though that cannot often be expected."

I shall offer one or two observations upon this statement, and I trust Mr. Children will believe that I do not object upon what appears to me frivolous grounds. In the first place, I think it more convenient to employ a solution of sulphuretted hydrogen in water than to pass the gas through the suspected solution; the same effects are produced, and to pass gas through so small a quantity of fluid as it is sometimes necessary to operate upon, increases the difficulty, but not the certainty of the process. It should be observed that no hydrosulphuret in solution should be employed instead of the mere gas; and the arsenious acid should be dissolved simply in water without the aid of any alkali; for this, if I remember rightly, interferes with the action of the sulphuretted hydrogen.

With respect to the employment of a nitric acid, in case phosphoric salts be present, I entertain considerable doubt. Nitric acid decomposes sulphuretted hydrogen, precipitating the sulphur, which may interfere with the process if the acid be employed in excess.

Mr. Children states that he finds that sulphuretted hydrogen gives a decided yellow colour to an ounce measure of distilled water, containing one drop of a saturated solution of arsenious acid, equal to $\frac{1}{180}$ th of a grain of the solid acid, or about $\frac{1}{270}$ th of the whole weight of the solution. Two or three drops of phosphate of soda prevented its action, but a little very dilute acetic acid immediately produced the yellow colour. Mr. Children properly observes that the acetic acid ought to be pure, and not such as has been distilled through a metallic worm.

It appears from this statement that the power of sulphuretted hydrogen as a test of arsenic is very great. It is singular that phosphate of soda should prevent its action; and I would submit, whether this very circumstance cannot be taken advantage of as an additional proof of the presence of arsenic.

In a work of this nature, embracing so many and such varied points of the science, it is not to be wondered at that some few inaccuracies should appear. As an example of these, I would

mention that Mr. Children does not seem to have, in all cases, clearly distinguished between chlorides and dry muriates, and the compounds of muriatic gas: thus, in p. 229, we are told that 100 parts of hydrochloric acid saturate 102 of lime. This must be what was formerly called dry muriatic acid—a substance of which Mr. Children will not admit the existence; but it will be observed in other cases, especially in the note at the bottom of this page, that Mr. Children evidently refers to hydrochloric acid, as a compound of hydrogen and chlorine. I confess I see no advantage in using the term *barya* instead of *baryta*, or even *barytes*; and if the term *hydrochloric acid* is to be admitted, I cannot conceive on what grounds *hydrosulphuric acid* for sulphuretted hydrogen is to be rejected.

I had intended to have made various other references to the useful additions contained in the Appendix; but I have already extended this article to so great a length that I must conclude with observing, that this work contains in a moderate compass what can scarcely be found without numerous references to a variety of chemical authorities; and I strongly recommend it as worthy of the confidence and study of the young analyst.—*Ed.*

Note by the Editor.—In the last number of the *Annals*, some errors occurred in the analysis of the Edinburgh Pharmacopœia.

In mentioning the *Oxidum Hydrargyri Cinereum*, the quantity of lime contained in the lime-water is much overrated. Under common circumstances, the quantity directed by the College cannot be considered as too large.

Page 61, line 10 from the bottom, for 42 read 32.

2 from the bottom, for one-ninth part read one-third part.

ARTICLE XV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

IN the last number of the *Annals*, the address of Sir Humphry Davy, on taking the chair as President, was given with a degree of brevity so little suited to the occasion, that I am happy to be able now to give more at length the heads of his discourse.

The President commenced his address by repeating his thanks to the Fellows of the Royal Society for the distinguished honour which they had done him by placing him in the chair. He stated his entire devotion to the cause of science, and assured the Society that his feelings were deep, and would be permanent.

Sir H. Davy then proceeded to point out the difficulties which attended the pursuits of philosophers at an early period, and hence existed the necessity of placing in the rooms of the Royal Society a collection of such machines as were useful in the progress of experimental knowledge. From the improvements which had been made in mechanical and chemical arts, he observed there were now but few occasions in which individuals could not conduct their experiments in their own laboratories; he expressed a hope, however, that on occasions of importance, and which might incur great expense, the proposers would not fail to recur to the Society.

The President then observed, that, owing to the progress of science, various associations had been formed for its advancement since the period when the Royal Society stood alone, and he expressed a hope that it would always preserve the most amicable relations with these new Societies; and that when any new facts of importance were observed by them, they would not fail to communicate them to the Royal, as the parent Society, whose records, he observed, contain all that was valuable from the time of our early philosophers. He disclaimed, however, all wish on the part of the Royal Society to exercise any authority over the more recent associations, whose objects were similar.

After some further remarks on the inexhaustibility of the subjects of scientific pursuit, he observed that philosophers, like the early cultivators in a great new continent, in proportion as they clear the country, discover more and more the vastness of the surrounding wilds. As the chart of a new country is essential in guiding the traveller, so, he continued, might the aspects and characters of new objects be useful to scientific investigation; and with this view the President offered some observations respecting those difficult departments of inquiry which appeared most capable of improvement. The pure mathematics, as a work of intellectual combination, are, he conceived, incapable of receiving aid from external phenomena; he considered them, however, at the present moment as promising new applications, observing that many departments of philosophical inquiry, to which the mathematics were formerly inapplicable, are now brought under its dominion.

After the discovery of the Georgium Sidus there appeared but little probability that new planetary bodies should be discovered nearer to our earth than any of those already known; yet this supposition, the President observed, had been found erroneous, owing to our limited conceptions of nature. The discovery of bodies smaller than satellites, but having the motions of primary planets, has opened new views of the arrangement of the solar system.

Sir H. Davy then, alluding to astronomy, as the most ancient

and perfect of the sciences, pointed out, as subjects for investigation, the nature of the systems of the fixed stars, their changes, the relations of cometary bodies to the sun, and the motions of those meteors which throw down showers of stones; for in a system, he observed, in which all is harmony, even these must be governed by fixed laws, and intended for definite purposes.

The great question of universal gravitation, and its connexion with the figure of the earth, the President observed, had been long solved. By the mechanical refinements of a Fellow of the Society, new means had been devised of estimating the force of gravity with exactitude. Sir H. Davy stated the desire which he knew was entertained by the Royal Academy of Sciences at Paris to connect their labours with those carried on by the command of the Board of Ordnance in Britain: should this take place, there would then be established, he observed, on the highest authority, an admeasurement of 1-18th of the whole circumference of the earth—a great record for posterity, and an honour to our own times.

As connected with the subject of the figure of the earth, Sir H. Davy referred to the late voyage to the Arctic regions, which, he observed, presented hopes of greater discoveries; and he made honourable mention of those by whom the expedition was planned and executed—an expedition which he characterized as worthy of the greatest maritime nation in the world.

The discoveries of Huygens, Newton, and Wollaston, in the theory of light and vision, were stated to have been followed by those of Malus. The President then alluded to the subject of the polarization of light, and the labours of other eminent philosophers; and expressed an opinion that this discovery would establish a new connexion between mechanical and chemical philosophy.

The subject of heat as connected with that of light was remarked as having lately afforded a rich harvest of discovery. The applications of the doctrine of heat to the atomic or corpuscular philosophy of chemistry were represented by the President as abounding in new views; and he noticed several facts which seemed to point to some general law on the subject: 1. The apparent equable motion of radiant matter, or light and heat, through space. 2. The equable expansion of all elastic fluids by equal increments of temperature. 3. The contraction or expansion of gases by chemical changes, in some direct ratio to their original volume. 4. The circumstance that the elementary particles of all bodies appear to possess the same quantity of heat.

The wonderful electrical instrument of Volta had done more, it was remarked, for the obscure parts of physics and chemistry, than the microscope for natural history, or even the telescope

for astronomy. Sir H. Davy then alluded to the electro-magnetic experiments of Oersted, and the award of the Copleian medal to that philosopher.

The subject of chemistry next claimed the attention of the President; and he justly remarked, that to point out all the objects worthy of inquiry in this branch of science would require many sittings of the Society. Among the more important desiderata, were mentioned the knowledge of the nature of the combinations of the principle of the fluor spar, and the metallization of ammonia, together with the connexion between mechanical and chemical phenomena in the action of voltaic electricity. Sir H. Davy then congratulated the Society on the rapid advances made in the theory of definite proportions, since it was first advanced in a distinct form by the ingenuity of Mr. Dalton, and he stated the promise which it affords of solving the recondite changes in the particles of matter, by laws depending upon their weight, number, and figure. As connected with definite proportions, the crystallizations, or regular forms of inorganic matter, were next noticed, and were observed to depend upon the motion of the combinations of elementary particles, to which the laws of electrical polarity and the polarisation of light seemed to have relation. Alluding to the difficulty of framing an hypothesis to account for the origin of the primary arrangements of the crystalline matter of the globe, Sir H. Davy stated the two principal facts which present analogies on the subject: One, that the form of the earth is that which would result, supposing it to have been originally fluid; and the other, that in lavas, masses decidedly of igneous origin, crystalline substances similar to those belonging to the primary rocks, are found in abundance.

The President then noticed the regular gradations which occur in the phenomena of nature, from the motions of the great masses of the heavenly bodies, to the imperceptible changes which produce the phenomena of crystallization; and when this ends, the series of animated nature governed by a distinct set of laws, begins; and as important objects of investigation, the functions and operations of organized beings were pointed out; as, for instance, those refined chemical processes by which the death and decay of one species afford nourishment for another and higher order; by which the water and inert matter of the soil and the atmosphere are converted into delicately organized structures, filled with life and beauty.

In vegetable physiology, the motion of the sap, the functions of the leaves, and the nature of the organs of assimilation, were mentioned as phenomena still remaining for investigation; and in animal physiology, the subjects were stated to be still more varied, obscure, and of a higher order; and a hope was expressed by the President, that the philosophers of the schools of Grewe and Hunter would not cease their efforts for the improvement of

these branches of science, the first of great utility to agriculture, and the latter, to medicine.

Sir H. Davy then expressed his conviction that the spirit of philosophy, awakened by the great masters, Bacon and Newton, would guide the future proceedings of the Fellows of the Royal Society. The sober and cautious method of inductive reasonings of these great philosophers, he described as the germ of truth and of permanency in all the sciences; and he trusted that those who were so fortunate as to kindle the light of new discoveries would use them, not for the purpose of dazzling the organs of intellectual vision, but rather to enlighten, by showing objects in their true form and colours.

A hope was also expressed by the President, that our philosophers will attach no undue importance to hypotheses, treating them rather as parts of the scaffolding of the building of science than as belonging to its foundations, materials, or ornaments; that they will look where it is possible, to practical applications in science, without forgetting the dignity of their pursuit, the noblest end of which is to exalt the powers of the human mind, and to increase the sphere of intellectual enjoyment, by enlarging our views of nature, and of the power, wisdom, and goodness, of the author of nature.

After alluding to the right which the Royal Society had to expect proofs of the zeal of those members in promoting its progress who had not yet contributed to it; and after stating that the Society would always consider the success of past, as pledges of future contributions, the President concluded a brief but luminous and impressive review of the present state of the sciences, with observing, "For myself, I can only say that I shall be most happy to give, in any way, assistance, either by advice, or experiments, in promoting the progress of discovery. And though your good opinion has, as it were, honoured me with a rank similar to that of a General, I shall be always happy to act as a private soldier in the ranks of science."

"Let us then labour together," said the President, "and steadily endeavour to gain what are, perhaps, the noblest objects of ambition—conquests in the field of natural knowledge; acquisitions which may be useful to our fellow creatures. Let it not be said that at a period when our empire was at the highest pitch of greatness, the sciences began to decline: let us rather hope that posterity will find in our records proofs that we were not unworthy of the times in which we lived."

Jan. 18.—A paper of Dr. Davy's was read, giving an account of his inquiries relative to the urinary organs, and secretion of two species of rana common in Ceylon: from which it appears, first, that the bladder of the bull frog and brown toad (the two species in question) is a genuine receptacle of urine, which it receives from the cloaca, in which the ureters terminate; and

secondly, that their urine is not at all analogous to that of other animals of the order amphibia, being very dilute, containing urea and certain salts, but no appreciable quantity of lithic acid. This peculiarity of urine, so well adapted to the size and structure of the bladder, is the more remarkable, as the favourite food of these animals is the same as that of small lizards, whose urine is of a butyraceous consistence, and nearly pure lithic acid. Hence, and from other facts mentioned by the author, he adduces the conclusion that the nature of urine, in every instance, depends much more on the peculiar action and structure of the secreting organs than on peculiarities of diet, or of the circulating fluids.

At the same meeting, a paper, by Capt. Kater, was read, entitled "An Account of the Comparison of various British Standards of Linear Measure."

Jan. 25.—The reading of the Bakerian lecture, by Captain Kater, on the form and the kinds of steel most proper to be employed in making magnetic needles, was commenced.

At the same meeting was read, "An Account of a Micro-meter made of Rock Crystal," by G. Dollond, FRS.

The improvement consists in making a sphere of rock crystal, and applying it in the place of the usual eye glass of a telescope, and from its natural double refracting property rendering it useful as a micrometer.

Jan. 5.—A paper on the Geology of the North-eastern Border of Bengal, by H. T. Colebrooke, Esq. VPGS. &c. was read.

The Brahmaputra river, which unites its stream with the Ganges at a short distance from their common junction with the sea, after a long course in the Himalaya, passes through the mountains of Aslam, and issues into the plain at the north-east corner of Bengal. At that position is a hill at Jogigopha, which is connected with the Rhotan mountains, and which consists chiefly of a large hemispherical mass of gneiss having strata, or rather masses, of granite on the north-eastern and western sides.

On the opposite or southern bank of the river is the hill of Pagnalath, which also appears to be composed of gneiss, the masses running from N.E. to S.W. at an angle of 45° .

At Givalpara, a few miles to the east of Paglanath, granite is found.

The same rocks occur again at Dhabui, a low hill, partly covered with alluvial soil, near the confluence of the Gadadhar. Blocks of primitive greenstone are also met with here in various parts of the bank of the river. At the confluence of the Kelanke river, which issues from the Garo hills, a little lower down is a precipitous bank, exhibiting graphic granite, and gneiss.

In the bed of the river, blocks of compact felspar, primitive greenstone, and quartz, united with felspar and hornblende, are found.

On the left bank of the Brahmaputra and the Caribari hills, or cliffs, which, for a considerable extent, consist generally of slate-clay, horizontally disposed, with a stratum of yellow (or more properly green) sand lying above it, indurated at the bottom in some places, and accompanied with ferruginous concretions. In many places, a stratum of clay is found resting on the green sand; and over it, the bank is composed of white or red sand mixed with gravel.

In different parts of the cliff, coarse-grained sandstone, clay ironstone concretions, nodules of slate clay and fossil wood have been found. In a bed of organic remains, situated under a small hill on the cliff, about seven feet below the level of the highest flood of the river, and 150 feet above the level of the sea, with layers of clay above and beneath, and resting upon alternate strata of sand and clay, a variety of fossils have been found, resembling in characters those which have been discovered in similar strata in the London and Paris basins.

On the banks of the Festa where it issues from the Rhotan mountains to descend into North Bengal, the rocks are found to consist principally of sandstone, containing much mica. Ferruginous sandstone was found in one place, and wood coal in another, where the sandstone comprises large pebbles. The banks of the Subeck, another river which descends from the Rhotan mountains, present similar strata.

ARTICLE XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. Crystallization of Red Oxide of Copper by Heat.

Mr. Chenevix, in his analysis of the arseniate of copper and of iron (Phil. Trans. 1801), has remarked, that by exposing oxide, hydrate, or carbonate of copper, without addition, to a violent heat, in an open crucible, he frequently obtained the red oxide of copper, presenting all the properties of this species of ore. On one occasion, he says, that the well-experienced eye of the Count de Bournon recognized a lump of it to be a mass of semi-fused artificial red copper ore.

Some time since I exposed a quantity of protoxide of copper to heat and air in an iron vessel with the intention of converting it into peroxide; I accidentally observed that the characters of the red oxide of copper were even more distinctly marked than they appear to have been in the mass obtained by Mr. Chenevix.

The bottom of the mass is in the form of that of the vessel in which the copper was burned—a portion of a sphere.

The mass consists of layers of pure copper, and of the red oxide,

alternating, but not with apparent regularity. The pure copper and red oxide are sometimes intermixed in the same layer.

The red oxide is distinctly, though minutely, crystallized in the form of the primary octohedron, and of the cube, and in intermediate varieties.

The pure copper is also in extremely small and brilliant crystals, but so minute that I cannot perceive their forms by the help even of the strongest glass.

The greater part of the surface of the mass is tinged of a green colour: portions of it are vitreous and transparent. The green colour is found, by the assistance of a strong glass, to be owing to the presence of fibrous crystals which are translucent, but too minute for the determination of their form. They have greatly the colour and appearance of the phosphate of lead.—Ed.

II. *Description of Two New Mineral Substances, found by Dr. Macculloch in Rum and Mull.*

“The last mineral substance,” says Dr. Macculloch, “which remains to be enumerated, being hitherto undescribed, it is necessary to enter into a minuter detail respecting it. Many years have passed since I discovered it both here (Rum) and in Fife; and the description has been hitherto delayed in hopes of finding larger specimens, better adapted for elucidating its history. These hopes have been hitherto disappointed, and I must therefore give it, however imperfect; in hopes that other mineralogists will hereafter supply the deficiencies. When recently broken it is of a green colour; varying from the transparent yellow green of the finest olivin (or chrysolite) which it sometimes resembles so as to be undistinguishable, to the dull muddy green of steatite, to which in this case it bears an equal resemblance. In a few hours after being taken from its repository, or exposed to the air, it turns darker, and shortly becomes black; a change which also occurs within the rock at the depth of an inch or more from the surface. In this case, the transparent variety puts on the external aspect with the lustre of jet: while the opaque one preserves its dull surface, and more nearly resembles black chalk. Notwithstanding this change, the mineral when in small fragments still continues to transmit light. The first variety remains perfectly translucent, and presents in some specimens the fine brown orange of cinnamon stone, in others a rich bottle or olive green. The other appears also of an olive green, but it is not more translucent than ore of the same thickness. When powdered, the one is of a snuffy brown, the other of a dirty olive. The structure of the first variety is generally conchoidal, that of the second is commonly intermediate between the conchoidal and granular. It is so soft as to be scratched by a quill, and is brittle; easily breaking into minute irregular fragments. The specific gravity is 2.020. With respect to its chemical habits, it remains unchanged before the blow-pipe; neither cracking nor sensibly altering its colour or translucency. It is apparently as refractory as quartz; a remarkable circumstance, when the quantity of iron in it is considered. It is acted on by muriatic acid, giving indications of a considerable proportion of iron, with a little alumina; but the principal constituent appears to be silica. There are no traces of lime or of manganese. The very minute quantity I

possessed for examination, prevents any more accurate detail of its composition.

"It is found imbedded in the amygdaloids of the cliffs of Scur more, the base being either a basalt; or a black indurated claystone. The nodules are generally round, and vary from the size of a radish seed to that of a pea or upwards. Occasionally they are oblong and compressed, and sometimes scale off in concentric crusts. In a few instances they are hollow within, the interior surface having a blistered aspect; or else the cavity of the amygdaloid is covered with the substance in a form resembling that of an exudation. More rarely still the nodule is compounded, containing a spherule of calcareous spar within an investing crust of the mineral. When long exposed to the air, it decomposes in the form of a rusty powder, which is thus occasionally found filling those cavities that are visible on the surface of the fragments in which it is found. The variety from Fife differs from that of Rum, in being less regular in form, and less frequently round, while it is commonly also of a larger size. From the most characteristic quality of this mineral, the term chlorophæite may be conveniently adopted for distinguishing it." Dr. Macculloch adds, that he has since seen this mineral brought from Iceland by Major Peterson. (*Western Islands*, vol. i. p. 504.)

"In reviewing some of the amygdaloids collected in Mull, I discovered a non-descript substance intermixed with some prehnite, but too small in quantity for examination. Having since found the same mineral in greater abundance in Glen Farg, I shall give the best description of it I am enabled to do from these specimens, as I cannot now assign the exact locality of the former. It has hitherto escaped the observation of mineralogists, and the description, however imperfect, will therefore be of use, by directing their attention towards it, and thus probably ascertaining its existence in other places. Its external characters are very limited, since it consists of a loose white powder, somewhat coarser than silica as it is obtained from the silicated alkalies, gritty between the teeth, but not so hard as to scratch glass.

"It does not effervesce with acids, and before the blow-pipe it melts immediately into a transparent colourless bead, with apparently the same facility as glass. It is certainly at least more fusible than datholite. I have not been able, from its condition, to determine its specific gravity. On attempting to analyze the very minute quantity that could be spared for that purpose, it was found to consist principally of silica. A small quantity of lime was taken up by muriatic acid, but its fusibility was not destroyed by that treatment. When treated, however, in a similar manner by sulphuric acid, the fusibility was destroyed. No alkali was found in it, nor any boracic acid, nor any traces of metallic matter. It is not easy therefore to account for its great fusibility, unless it should contain the new alkali. In this uncertain state must its chemical composition remain, until other specimens are procured to admit of a repetition of these experiments on a more extensive scale. It is found filling irregular cavities in the amygdaloid of the valley above-mentioned, so well known to mineralogists; where it is accompanied by analcime, mesotype, prehnite and calcareous spar. There is no appearance of decomposition in the

accompanying minerals, and I may add that the specimens in question were broken from a fresh rock in which they were deeply imbedded. The preceding characters seem to distinguish it for the present from all the mineral substances hitherto described; and the term *conite* appears well adapted for it, as being expressive of its most conspicuous feature.

“On this substance I shall only remark as I recently did on treating of Rum, that it is preferable to erect a species, though it should afterwards prove a variety of some known substance, than to neglect the obscure characters which minerals often present. It is to greater accuracy of research and of knowledge that we are indebted to the recent rapid augmentation of the list of minerals.” (Vol. i. p. 578.)

III. *Production of Artificial Cold.*

When on Ben More in Mull, about 3097 feet high, Dr. Macculloch made the following observation.—“On this mountain I was accidentally led to observe the degree of cold produced by the mixture of ice and alcohol. A storm of hail had fallen, accompanied by a temperature below freezing. Some whiskey, the usual appendage of a Highland viaticum, being produced, I was obliged to dilute it by putting some hail into the cup. In an instant the metal was covered with ice, and frozen to the glass: on trial, the quicksilver in the thermometer sunk into the bulb. On repeating the experiment afterwards with common alcohol, the cold was found to amount to 49° or 50° . It presents a convenient method of obtaining a low temperature, when other less common materials are not at hand.” (Western Islands, vol. i. p. 534.)

IV. *Mercurial Atmosphere.*

It has been long admitted that in the upper part of the thermometer and barometer an atmosphere of mercury exists, having a very small degree of tension; and Mr. Faraday has shown, by the following simple experiment, that a mercurial atmosphere may exist without removing the air. A small portion of mercury was put through a funnel into a clean dry bottle, capable of holding about six ounces, and formed a stratum at the bottom, not one-eighth of an inch in thickness; particular care was taken that none of the mercury should adhere to the upper part of the inside of the bottle. A small piece of leaf-gold was then attached to the under part of the stopper of the bottle; so that when the stopper was put into its place, the leaf-gold was inclosed in the bottle. It was then set aside in a safe place, which happened to be both dark and cool, and left for between six weeks and two months. At the end of that time, it was examined, and the leaf-gold was found whitened by a quantity of mercury, through every part of the bottle, and mercury remained apparently just as before.

This experiment was repeated several times, showing that mercury is always surrounded by an atmosphere of the same substance.—(Faraday, Quarterly Journal of Science, No. xx. p. 355.)

V. *Sulphuret of Chrome.*

Mr. I. L. Lassaigne has obtained this compound by the following process: Chloride of chrome was prepared by boiling chromic acid with excess of muriatic and evaporating to dryness. It was then mixed

with five times its weight of sulphur, and heated to whiteness in a bent glass tube. The resulting sulphuret of chrome was blackish-grey colour; it is unctuous to the feel, very light, and readily falling to powder; when rubbed on bodies, it leaves marks similar to those of plumbago. Heated in a platina crucible, it burns like pyrophorous, giving fumes of sulphurous acid, and leaving deep-green coloured oxide of chrome. It is not readily acted upon by nitric acid: but when this acid is mixed with muriatic, it is easily dissolved. It is composed of chrome, 100; and sulphur, 10.54. By mixing equal parts of chromate of potash and sulphur, and heating the mixture in close vessels, green oxide of chrome was economically obtained by M. Lassaigue. It is to be washed with water to dissolve the sulphate and sulphuret of potash, which leaves the oxide of chrome pure. Equally fine oxide of chrome was obtained by heating sulphur with the produce of the evaporation of the solution of chromate of iron, treated by nitre, to which a little sulphuric acid had been previously added to precipitate the earthy matters which had been dissolved.

Owing to a mistake of the engraver, it has been found requisite to give a new Plate of Mr. Pratt's Clinometer, which the reader is requested to substitute for that in the last number of the Annals.

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Good is preparing for publication, *The Study of Medicine*, comprising its Physiology, Pathology, and Practice. These volumes, in addition to that lately published on Nosology, and dedicated, by permission, to the College of Physicians, will complete the Author's design; and constitute an entire body of Medical Science, equally adapted to the use of Lecturers, Practitioners, and Students.

Travels in Syria and Mount Sinai, by the late J. L. Burckhard, are in the press.

A work, entitled "*Practical Economy, or Hints for the Application of Modern Discoveries to the Purposes of Domestic Life*," is preparing for publication.

Mr. Haden, of Sloane-street, is about to publish a *Monthly Journal of Medicine*, addressed principally to unprofessional persons. The work will teach the prevention rather than the cure of disorders; at the same time that it will point out how the friends of the sick may, in the best way, assist their medical men in his treatment, and otherwise show how health may be preserved, and disease warded off.

JUST PUBLISHED.

An Inquiry into the Nature and Treatment of Gravel, Calculus, and other Diseases connected with a deranged Operation of the Urinary Organs. By William Prout, MD. FRS. 8vo. 7s. boards.

An Essay on Sea Bathing, in preserving Health, and as a Remedy in Disease, especially Nervous, Scrophulous, &c. By J. W. Williams, Member of the College of Surgeons, London. 12mo. 5s. 6d.

Cases illustrating the improved Treatment of Stricture in the Urethra and Rectum. By James Arnott. 8vo. 4s. 6d.

A Dictionary of Chemistry, on the Basis of Mr. Nicholson's; in which the Principles of the Science are investigated anew. By Andrew Ure, MD. 8vo. 1l. 1s. boards.

Practical Observations in Midwifery; with a Selection of Cases. By John Ramsbotham, MD. 8vo. Part I. 10s. 6d.

Practical Observations on Chronic Affections of the Digestive Organs, and on Bilious and Nervous Disorders. By John Thomas, MD. 8vo. 6s.

Illustrations of the great Operations of Surgery. By Charles Bell. Part I. Plates coloured. 1l. 1s.

Universal Science; or the Cabinet of Nature and Art; comprising various Selections from useful Discoveries in the Arts and Sciences. By Alex. Jamieson. 2 vols. 12mo. 16s.

ARTICLE XVIII.

NEW PATENTS.

James Ransome, of Ipswich, iron-founder, and Robert Ransome, of Colchester, iron-founder, for an improvement upon an invention by James Ransome, for which he now hath a patent, June 1, 1816, for certain improvements on ploughs. Nov. 28, 1820.

William Kendrick, of Birmingham, chemist, for a combination of apparatus for extracting a tanning matter from bark and other substances containing such tanning matter. Dec. 5.

Thomas Dolbs, of Smallbrook-street, for a mode of uniting together, or plating, tin upon lead. Dec. 9.

John Moore, Jun. of Castle-street, Bristol, for a certain machine or machinery, which may be worked by steam, by water, or by gas, as a moving power. Dec. 9.

George Vaughan, of Sheffield, for a blowing machine, on a new construction, for the fusing and heating of metals, smelting of ores, and supplying blast for various other purposes. Dec. 14.

William Mallet, of Marlborough-street, Dublin, for improvements on locks, applicable to doors, and to other purposes. Dec. 14.

Andrew Timbrell, of the Old South Sea House, London, for an improvement of the rudder and steerage of a ship or vessel. Dec. 22.

Sir William Congreve, Bart. of Cecil-street, Strand, for improvements in printing in one, two, or more colours. Dec. 22.

William Pritchard, of Leeds, for improvements in an apparatus to save fuel, and for the more economical consumption of smoke in shutting fire-doors and air-flues in steam-engine boilers, drying-pans, and brewing-pans, other fire-doors and air-flues. Dec. 22.

Marc Isambard Brunel, of Chelsea, for a pocket copying-press, and also improvements on copying-presses. Dec. 22.

ARTICLE XIX.

Astronomical, Magnetical, and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37' 44.3" North. Longitude West in time 1' 20.93".

Astronomical Observations.

Dec. 2. Emersion of Jupiter's second	{	7 ^h 32' 02"	Mean Time at Bushey.
satellite.		7 33 23	Mean Time at Greenwich.

Magnetical Observations, 1820. — Variation West.

Month.	Morning Observ.				Noon Observ.				Evening Observ.	
	Hour.	Variation.			Hour.	Variation.			Hour.	Variation.
Dec.	1	8 ^h 50'	24°	32'	24"	1 ^h 20'	24°	36'	44"	Owing to the shortness of the days, evening observations discontinued.
	2	8 45	24	35	34	1 15	24	36	25	
	3	—	—	—	—	1 30	24	36	42	
	4	8 35	24	31	40	1 20	24	37	26	
	5	8 40	24	32	26	1 25	24	36	45	
	6	8 55	24	33	56	1 40	24	37	14	
	7	8 40	24	32	18	1 20	24	36	42	
	8	—	—	—	—	—	—	—	—	
	9	8 40	24	33	19	1 10	24	36	22	
	10	8 35	24	32	51	1 15	24	37	21	
	11	8 45	24	39	35	1 40	24	39	49	
	12	—	—	—	—	1 20	24	36	20	
	13	8 45	24	35	15	1 25	24	36	46	
	14	8 45	24	32	27	1 15	24	36	59	
	15	8 35	24	32	44	—	—	—	—	
	16	8 40	24	32	37	1 10	24	37	00	
	17	—	—	—	—	1 20	24	35	38	
	18	—	—	—	—	—	—	—	—	
	19	—	—	—	—	—	—	—	—	
	20	—	—	—	—	1 15	24	37	09	
	21	8 50	24	33	30	1 25	24	36	24	
	22	8 45	24	32	12	1 15	24	37	46	
	23	8 50	24	32	20	1 10	24	36	19	
	24	8 45	24	31	53	1 15	24	35	58	
	25	8 40	24	31	56	1 20	24	36	17	
	26	8 55	24	32	29	1 25	24	36	05	
	27	8 55	24	32	44	1 20	24	36	18	
	28	8 45	24	32	34	1 15	24	36	42	
	29	8 45	24	31	17	1 20	24	36	37	
	30	8 50	24	32	50	1 20	24	35	51	
	31	8 45	24	32	10	1 15	24	35	55	
Mean for Month.	8 45	24	33	03	1 20	24	36	34		

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six'a.
Dec.		Inches.				Feet.		
1	Morn....	29·676	35°	78°	WNW		Cloudy	33½
	Noon....	29·649	41	70	W		Cloudy	41
	Even....	—	—	—	—		—	36
2	Morn....	29·449	37	80	W by S		Cloudy	40½
	Noon....	29·449	40	69	W		Cloudy	40½
	Even....	—	—	—	—		—	34
3	Morn....	29·603	—	82	SSW		Fog, rain	43
	Noon....	29·579	43	75	SW by W		Rain	43
	Even....	—	—	—	—		—	49
4	Morn....	29·479	49	78	W by N		Cloudy	53
	Noon....	29·483	52	72	W by S		Fine	48
	Even....	—	—	—	—		—	51
5	Morn....	29·440	49	71	W		Cloudy	45
	Noon....	29·382	50	70	W		Rain	50
	Even....	—	—	—	—		—	45
6	Morn....	29·541	45	84	E		Fog	50
	Noon....	29·574	46	79	SE		Rain, fog	45
	Even....	—	—	—	—		—	49
7	Morn....	29·563	49	79	W		Fine	53
	Noon....	29·589	52	73	W by S		Cloudy	50
	Even....	—	—	—	—		—	45½
8	Morn....	29·682	—	74	W by S		Cloudy	48½
	Noon....	—	—	—	—		—	45
	Even....	—	—	—	—		—	48½
9	Morn....	29·703	47	69	W by S		Cloudy	45
	Noon....	29·660	48	68	W by S		Cloudy	48½
	Even....	—	—	—	—		—	45
10	Morn....	29·543	48	74	SW by W		Cloudy	51½
	Noon....	29·515	49	78	SW by W		Rain	48
	Even....	—	—	—	—		—	51½
11	Morn....	29·375	49	82	WSW		Rain	47
	Noon....	29·412	51	72	W		Mixale	51½
	Even....	—	—	—	—		—	46
12	Morn....	29·131	—	87	SSW		Fog, rain	51½
	Noon....	29·100	50	76	WSW		Cloudy	46
	Even....	—	—	—	—		—	30½
13	Morn....	28·932	46	82	NNE		Fog, rain	36½
	Noon....	28·990	36	82	NE		Rain	30
	Even....	—	—	—	—		—	36½
14	Morn....	29·396	31	70	N by E		Clear	30½
	Noon....	29·429	36	67	NNE		Clear	32½
	Even....	—	—	—	—		—	26
15	Morn....	29·464	32	65	ESE		Cloudy	34
	Noon....	—	—	—	—		—	30
	Even....	—	—	—	—		—	40
16	Morn....	29·193	28	74	ESE		Cloudy	36
	Noon....	29·093	31	71	ESE		Cloudy	45
	Even....	—	—	—	—		—	—
17	Morn....	29·127	34	83	Calm		Fog	—
	Noon....	29·194	38	78	N by W		Fog	—
	Even....	—	—	—	—		—	—
18	Morn....	29·600	—	87	SE by E		Fog	—
	Noon....	29·624	—	83	SE		Fog	—
	Even....	—	—	—	—		—	—

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six'ts.
Dec.		Inches.				Fect.		
19	Morn....	29.751	—	88°	S		Fog, rain	42
	Noon....	29.768	—	85	SW		Fog, rain	48½
	Even....	—	—	—	—		—	—
20	Morn....	29.900	—	84	SSE		Wet, fog	39
	Noon....	29.855	43	88	S		Fog, rain	48½
	Even....	—	—	—	—		—	—
21	Morn....	29.618	47	73	W		Cloudy	42
	Noon....	29.638	46	69	W		Cloudy	48
	Even....	—	—	—	—		—	—
22	Morn....	29.688	37	78	W by N		Very fine	36½
	Noon....	29.648	42	72	W		Rain	42½
	Even....	—	—	—	—		—	—
23	Morn....	29.510	39	75	NE		Cloudy	37½
	Noon....	29.458	44	68	NE		Cloudy	41
	Even....	—	—	—	—		—	—
24	Morn....	29.400	32	73	E by N		Cloudy	31½
	Noon....	29.390	32	70	ENE		Cloudy	32
	Even....	—	—	—	—		—	—
25	Morn....	29.349	29	70	ENE		Cloudy	28
	Noon....	29.335	29	71	ENE		Cloudy	29½
	Even....	—	—	—	—		—	—
26	Morn....	29.263	28	72	E		Rain	26
	Noon....	29.263	29	70	E		Cloudy	30
	Even....	—	—	—	—		—	—
27	Morn....	29.394	28	69	E by N		Cloudy	27½
	Noon....	29.402	29	69	E		Cloudy	30
	Even....	—	—	—	—		—	—
28	Morn....	29.404	27	69	NE by E		Clear	27
	Noon....	29.434	29	63	NE by E		Cloudy	29½
	Even....	—	—	—	—		—	—
29	Morn....	29.479	24	66	NE by		Cloudy	23½
	Noon....	29.438	24	66	NE by		Cloudy	25
	Even....	—	—	—	—		—	—
30	Morn....	29.410	24	68	NE		Cloudy	23½
	Noon....	29.429	25	65	NE		Fine	26
	Even....	—	—	—	—		—	—
31	Morn....	29.432	21	70	NE		Cloudy	20
	Noon....	29.391	27	65	NE		Very fine	27½
	Even....	—	—	—	—		—	—

Rain, by the pluviometer, between noon the 1st of Dec. and noon the 1st of Jan. 1.468 inch. Evaporation, during the same period, 1.133 in.

ARTICLE XX.

METEOROLOGICAL TABLE.

1820.	Wind.		BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a.m.
			Max.	Min.	Max.	Min.			
12th Mon.									
Dec.	1	N W	30.17	29.99	42	37	—	—	90
	2	N W	30.10	29.95	42	31	—	.03	84
	3	S W	30.10	29.97	50	38	—	—	90
	4	W	29.97	29.93	53	37	—	—	77
	5	W	30.03	29.87	51	45	—	.06	69
	6	E	30.03	30.02	51	44	—	.04	94
	7	N W	30.12	30.02	55	49	—	—	82
	8	S W	30.15	30.12	50	45	—	—	76
	9	W	30.15	30.05	50	47	—	—	64
	10	S W	30.05	29.90	52	50	—	—	86
	11	W	29.90	29.65	53	48	—	.25	74
	12	S W	29.65	29.49	54	47	—	—	90
	13	N W	29.90	29.47	46	29	—	.78	94
	14	N E	29.98	29.90	39	30	—	—	84
	15	S E	29.90	29.79	34	26	—	—	62
	16	S E	29.79	29.73	35	30	—	—	78
	17	E	30.10	29.73	41	34	—	.40	91
	18	E	30.23	30.10	57	41	.56	.02	94
	19	S	30.35	30.23	50	36	—	—	100
	20	S	30.35	30.11	50	40	—	.05	100
	21	W	30.15	30.09	50	33	—	—	71
	22	S W	30.09	30.01	43	34	—	.04	88
	23	N	30.01	29.95	42	33	—	—	90
	24	N E	29.95	29.93	35	30	—	—	64
	25	N E	29.93	29.88	32	27	—	—	67
	26	E	29.96	29.88	32	29	—	—	70
	27	E	29.99	29.96	35	28	—	—	65
	28	E	30.04	29.99	33	25	—	—	61
	29	E	30.04	30.03	29	24	—	—	59
	30	E	30.03	30.02	29	22	—	—	60
	31	N E	30.02	29.97	30	21	.45	—	58
			30.35	29.47	57	21	1.01	1.67	100—58

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Twelfth Month.—1, 2. Cloudy. 3. Cloudy: windy night. 4. Fine. 5. Cloudy. 6—9. Cloudy. 10. Overcast: some rain. 11. Overcast. 12. Cloudy: rainy night. 13. Showers, with gusts of wind most of the day: some sleet at one, p.m.: about half-past eight, p.m. a lunar corona surrounded by a double-coloured halo. 14. Fine. 15. Windy: bleak. 16. Rain: sleet: snow: boisterous. 17. Gloomy: ground covered with snow in the morning. 18. Gloomy: the snow nearly all gone. 19. Foggy night: a faint lunar halo, of the largest diameter, about eight, p.m. 20. Gloomy. 21. Gloomy: foggy. 22. Foggy morning: cloudy. 23. Overcast: drizzling. 24. Bleak. 25. Cloudy: bleak. 26, 27. Bleak and cloudy. 28. Fine clear morning: very cold wind. 29, 30. Cloudy: very cold and boisterous wind. 31. Overcast: cold wind.

RESULTS.

Winds: N, 1; NE, 4; E, 8; SE, 2; S, 2; SW, 5; W, 5; NW, 4.

Barometer: Mean height

For the month..... 29.983 inches.

For the lunar period, ending the 27th..... 30.002

For 13 days, ending the 12th (moon south) 29.990

For 14 days, ending the 26th (moon north) 29.981

Thermometer: Mean height

For the month..... 39.241°

For the lunar period..... 40.645

For 29 days, the sun in Capricornus 42.462

Evaporation..... 1.01 in.

Rain..... 1.67

Mean of hygrometer..... 78°

Laboratory, Stratford, First Month, 16, 1821.

R. HOWARD.

ANNALS OF PHILOSOPHY.

MARCH, 1821.

ARTICLE I.

On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints which many of them develope on Exposure to a polarised Ray. By J. F. W. Herschel, Esq. FRS. Lond. and Edin. (With a Plate.)

(Concluded from p. 132.)

OUR equation (b) gives room for a remark of some consequence, as it affords a striking verification of the theory here delivered. It will be observed that this equation does not involve t , and in consequence, the angle θ determined from it, at which the coincidence takes place, is the same for all values of t , or for all thicknesses of the plate. The observations of the tints in the tables given above afford us ample means of putting this remarkable consequence to the test of experiment. In fact, in the three series of tints observed in sulphate of baryta, the apparent angles between the axes for the mean red rays are respectively $62^{\circ} 0'$, $62^{\circ} 2'$, and $61^{\circ} 53'$, the mean of which is $61^{\circ} 58'$, while the apparent angles between the virtual poles in the same series are $72^{\circ} 46'$, $72^{\circ} 36'$, and $72^{\circ} 47'$. The semi-excesses of the latter angles over the mean value of the former are the apparent angular distances of the virtual poles from the axes of mean red rays, and are respectively $5^{\circ} 24'$, $5^{\circ} 19'$, $5^{\circ} 25'$, neither of which differs more than $4'$ from the mean.

To ascertain the real value of θ , we have only to compute the angles of refraction. In the specimen employed, I found 1.6475 for the index of ordinary refraction, and the angles of incidence

(the halves of the above angles) being $30^{\circ} 59'$, and $36^{\circ} 23'$, $36^{\circ} 18'$, $36^{\circ} 24'$; the corresponding angles of refraction are $18^{\circ} 12' 30'' (= a)$, $21^{\circ} 6' 10''$, $21^{\circ} 3' 40''$, $21^{\circ} 6' 40''$; and since $\theta = \phi - a$ in this case, we find for the values of θ , $2^{\circ} 53' 40''$, $2^{\circ} 51' 10''$, $2^{\circ} 54' 10''$, the mean of which gives $2^{\circ} 53' 0''$ for the real angular distance of the virtual pole from the axis of mean red rays in sulphate of baryta.

Again, in the series of tints, Tables V, VI, VII, for Rochelle salt, the apparent angular distances of the mean red axis from the virtual pole were $277^{\circ} 8' - 257^{\circ} 35' = 19^{\circ} 33'$; $281^{\circ} 30' - 262^{\circ} 0' = 19^{\circ} 30'$, and $282^{\circ} 0' - 262^{\circ} 25' = 19^{\circ} 35'$, of which neither differs more than $3'$ from the mean $19^{\circ} 33'$.

Dr. Brewster (Phil. Trans. 1814, p. 216) has stated the refractive index of this salt at 1.515; but this is certainly a little too large. In four experiments made at distant intervals of time, and by different modes of observation, I have found

1.49640

1.50220

1.49670

1.49853

for the index for the mean yellow-green rays, of which determinations the last is to be preferred, having been made with great care. The same experiment gave 1.49293 for the index for mean red rays. The apparent angular distance of the axis for red rays from the perpendicular was 16° , which leaves $3^{\circ} 33'$ for the angular distance of the virtual pole from the perpendicular. These angles of incidence correspond to the respective angles of refraction $10^{\circ} 38' 20''$ and $2^{\circ} 22' 40''$, of which the sum $13^{\circ} 1'$ is the real angle between the virtual pole and mean red axis in Rochelle salt. The series in Table IV. gives $13^{\circ} 2' 25''$ for the value of θ , which agrees completely with the foregoing determination.

I took seven plates of nitre of various thicknesses, and cut from different crystals, and by a mode of observation to be described hereafter, found as follows :

TABLE VIII.

Designation of the plate.	Mean distance of the virtual poles from the axis of mean yellow rays (apparent).	Excess above the mean.	Order of the coincidences.
1	$1^{\circ} 50''$	-4'	Between the fourth minimum and fifth maximum
2	$1^{\circ} 51''$	-3'	A little beyond the third maximum
3	$1^{\circ} 57''$	+3'	Different at the two extremities
4	$1^{\circ} 58''$	0	Second maximum
5	$1^{\circ} 55''$	+2'	Second minimum
6	$1^{\circ} 52''$	+5'	Ninth minimum
7	$1^{\circ} 53''$	0	A little beyond the eighth minimum
Mean	$1^{\circ} 53''$		

Although the constancy of the position of the virtual pole for different thicknesses is sufficiently made out here, the small differences which exist are certainly not attributable to errors of observation, which, in the method I employed, are usually confined within much narrower limits. They are due to minute irregularities in the crystals themselves, consisting probably in a state of imperfect equilibrium of the molecular forces of aggregation, to which this salt is so subject, that it is rather rare to find a specimen in which the rings beyond both poles have exactly the same breadth or tints.

IV. *Of the Tints developed by Crystals with two Axes out of the principal Section.*

If we place a crystallized plate at an azimuth zero in a tourmaline apparatus, having the axes of the tourmalines at right angles, we shall observe, if its thickness be at all considerable, that the two oval spots on either side the axis of symmetry (which is now perfectly black) instead of being exactly regular in their figure, as in Pl. IV. fig. 2, and tinged with colours symmetrically disposed on either side of a line $m n$ perpendicular to the principal section, are invariably coloured at one extremity r with a strong prismatic red hue, and drawn out at the other v into more or less elongated and tapering spectra or tails of blue and violet light. The extremities r, r of the rings too have a large excess of the red rays, and the opposite v, v of the violet rays. In crystals of the first class above described, the red extremity is turned towards the other pole, while in those of the second it is directed from it. If we subject a plate of Rochelle salt to this examination, the ovals a, a , are drawn out to a surprising length, and the whole prismatic spectrum is displayed in them with great vividness of colour, while the violet portions of the rings are greatly elongated also, and appear to run into one another. If the plate be turned round in azimuth, the phenomena assume the most singular appearances of distortion; and as the rotation approaches to 45° , the rings in the vicinity of the pole are gradually obliterated by their mutual overlapping, which is the greater the thicker the plate. In all situations, however, the interposition of a red or dark green glass immediately restores the perfect symmetry and regularity of the rings, which are then seen in much greater number, and completely well defined.

All this is the necessary consequence of the want of coincidence of the axes for different colours. The lateral spots, for example, are formed for each homogeneous colour with perfect regularity close to their corresponding pole, and regularly decreasing in size from the red to the violet. Their arrangement will, therefore, be as represented in fig. 3, R, O, Y, &c. being the poles corresponding to the several colours, red, orange, &c. The oval spots composed of red rays being represented by r, r , those of the other colours will be super-imposed on them in their

order, overlapping, as represented by the dotted ovals *pq, xy*, &c. like the circular coloured images of the sun in the spectrum of an ordinary prism, giving rise to the long prismatic tails above described. Similar considerations will apply to the anomalous appearances presented by the rings of all the other orders, and in every situation.

This suggests a very simple and pleasing experiment, which affords an ocular demonstration of the adequacy of the explanation I have advanced. Let a plate of Rochelle salt be placed in a tourmaline apparatus in any azimuth (45° is the most convenient) and firmly fixed on a proper stand in a dark room. The eye being now applied, let an assistant illuminate the emetied glass or lens of short focus* which disperses the light previous to its incidence on the first tourmaline, with the several colours of the prismatic spectrum in succession, beginning with the red. The rings will then be seen formed successively of each of the colours, perfectly regular in their figure, but contracting rapidly in dimension as they become illuminated with the more refrangible rays.† At the same time the pole about which they form will be seen to move regularly in the direction of the other axis of the crystal, and if we pass alternately from a red to a violet illumination, will shift its position accordingly, backwards and forwards, through a very considerable angle. If rays of two colours be thrown at once on the apparatus, their two corresponding sets of rings will be seen at once, crossing, but not obliterating one another, and the distance between their respective centres will be observed to increase with the difference of their colours.

By measuring the interval occupied by the projections of the centres of the last visible red and violet rings, as well as those of the intermediate colours, on a screen at a known distance seen with the other eye, I found the following values of the apparent and real separation of the several coloured axes :

	Between	Apparent interval.	Real interval.
Red and orange	0° 37"	very uncertain	0° 25"
yellow	1 50"	rather uncertain	1 13"
green	3 43"	ditto	2 29"
blue	6 18"	ditto	4 3"
indigo	8 19"	ditto	5 33"
violet	9 46"	ditto	6 31"

(*) As a mode of measurement this method is very inaccurate,

* See the description of an apparatus of this kind, subjoined.

† See *Lectures on Optics*, lib. ii. Pars. i. Obs. xiii. from which the idea of this experiment is taken. "Magnaque voluptate perfusus," says Newton, with the enthusiasm of the true philosopher who loves the field he labours in, "videbam eos diffundere et contrahere se gradatim pro eo ac color luminis immutabatur." (It is impossible to witness the very beautiful phenomenon described in the text without entering into the same train of feeling.)

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 especially in the extreme red and violet rays, both of which would be copiously, and indeed almost entirely absorbed in their passage through two plates of tourmaline of a yellowish-green colour. Much more exact and unexceptionable measures will be presently given, but these are quite sufficient to establish the reality of the phenomenon described.

V. *Of a secondary Cause of the Deviation of Tints, subsisting in certain Crystals, and of the anomalous Tints of the Apophyllite.*

To determine the dispersive power of any medium, and obtain some rough knowledge of its law, we make a prism of it act in opposition to one of a standard substance. To ascertain the total dispersion of the axes of a crystal, or the angle by which the extreme red and violet axes differ, we may make it act against itself. Since the violet rings are more elevated by refraction than the red, from the situation in which they would appear to an eye immersed in the medium, a plate may be conceived cut in such a direction as to make their apparent centres coincide, in which case the tints immediately about the poles will coincide with Newton's scale, and the extraordinary image will totally disappear in the pole at an azimuth 45° . This condition gives $\theta = 0$, $\theta - \delta a + \delta \phi = 0$, whence (supposing R , R' the indices of refraction for extreme red and violet rays, and $\delta R = R' - R$) we find

$$\delta a = \delta \phi = - \frac{\delta R}{R} \cdot \tan. \phi$$

The angle ϕ however becomes imaginary, and this method, in consequence, inapplicable when the separation of the extreme axes (δa) is greater than the maximum dispersion of the colours of an intromitted white ray, that is, when

$$\delta a > \frac{\delta R}{R \cdot \sqrt{R^2 - 1}}$$

Let us resume our equation (b), and supposing the form of the function ψ , and the constants a , k , k' , R and δR ascertained, let the angle θ , at which the coincidence takes place be observed, and the value of δa will then become known. If we suppose it small, which it is in the generality of crystals, we get

$$\delta a = \frac{\frac{k - k'}{k'} \cdot \psi - \delta \phi \cdot \left\{ \frac{k}{k'} \cdot \sin. \phi \cdot \psi + \frac{d\psi}{d\theta} + \frac{d\psi}{d\theta'} \right\}}{\frac{d\psi}{d\theta} - \frac{d\psi}{d\theta'}}; \quad (c)$$

(ψ being put for $\psi(\theta, \theta')$ for the sake of brevity). At incidences nearly perpendicular, $\delta \phi$ may be neglected, and the expression reduces itself to

$$\delta a = \frac{k - k'}{k'} \cdot \frac{\psi}{\frac{d\psi}{d\theta} - \frac{d\psi}{d\theta'}} \quad (d)$$

The comparison of these formulæ with observation, which

will lead to some very remarkable consequences, requires us to know the form of the function ψ and the values of k , k' . We will begin with the former, and in this investigation the first step is to determine the general equation of the isochromatic lines. In order to this, we must separate in all cases the law of the tint from that of its intensity. The latter depends entirely on the greater or less facility which the emergent ray finds in penetrating the prism of Iceland spar employed for its analysis, and will not enter into the present investigation. When we examine a crystallized plate in a convenient graduated apparatus between tourmaline plates crossed at right angles, turning it slowly round between them in its own plane, the form of the coloured bands, if illuminated with homogeneous light, will remain perfectly unchanged during the rotation, but the two black hyperbolic branches passing through the poles will obliterate in succession every part of their periphery; and the space over which the darkness extends, as well as the degree of illumination of what remains visible, varying at every instant, give rise to so great a variety of appearances, that some little attention is required to recognize this perfect identity of figure. When the tourmaline next the eye is made to revolve, the crystallized plate remaining fixed, the complicated changes which take place, are perfectly reconcilable with the superposition of the primary on its complementary set of rings, the relative intensities of the two sets at any point being regulated by laws; we have no occasion to consider at present, but the figure of the isochromatic lines, where visible, remains absolutely unchanged by any rotation in this part of the apparatus.

To form a first hypothesis on the nature of the function which determines the equation of any one of these curves, we must select a crystal, where the proximity of the axes and intensity of the polarising forces are such as to bring the whole system of rings within a very small angular compass; as by this means we avoid almost entirely the disturbing effect of the variation in thickness, arising from obliquity of incidence. Dr. Brewster, in his paper of 1818, has chosen nitre, as affording the best general view of the phenomena, and it is admirably adapted for this purpose; the whole system of rings being comprised at a very moderate thickness within a space of 10° , allowing us to regard their projection on a plane perpendicular to the optic axis as a true representation of their figure, undistorted by refraction at the surface, &c. If we examine the rings in this crystal (illuminated with homogeneous light, or by the intervention of a red glass in common day-light), it will be evident that the general form of any one of them is a re-entering symmetrical oval, which no straight line can cut in more than four points, and which, by a variation of some constant parameter, is in one state wholly concave, as 1, fig. 4, then becomes flattened, as 2; then acquires a minimum ordinate and points of contrary flexure, as 3;

then a node, as 4; after which it separates into two conjugate ovals, as 5; which ultimately contract themselves into the poles P, P' as conjugate points. The general idea bears a striking resemblance to the variation in form of the curve of the fourth order, so well known to geometers under the name of the lemniscate, whose equation is

$$(x^2 + y^2 + a^2)^2 = a^2(b^2 + 4x^2)$$

when the parameter b gradually diminishes from infinity to zero, $2a$ being the constant distance between the poles. In order, however, to put this to a satisfactory examination, accurate measures must be taken, which, in the case of nitre, from the minuteness of the system of rings, presented at first some difficulties. These I obviated, after many fruitless trials, by a mode of observation which I have found extremely convenient and accurate, and which applies particularly well to the present purpose. It consists in projecting the rings by solar light on a screen in a darkened room, by which means they may be magnified to any required extent, examined at perfect leisure, and in all their phases, and measured or traced with a pencil with the utmost exactness and facility. They may be thus exhibited to a number of spectators at once; a thing which may prove serviceable to the lecturer, for which reason I have subjoined to this paper a brief description of the apparatus I employ.

Having cut a very perfect crystal of nitre at right angles to its axis of crystallization, and adjusted it properly on this apparatus, the rings were projected on a large sheet of paper, stretched, while moist, on a drawing board, by which means it assumes a truly plane surface by the contraction it undergoes while drying. The poles were then marked, and the loci of the successive maxima for red rays carefully outlined. The screen being then removed, a series of lemniscates were laid down by points, having the same poles, and one common point in each, chosen where the tint was most decided. It is unnecessary to give any comparative statement of measures in the observed and constructed curves, as the points, graphically laid down, uniformly fell on the pencilled outlines, or, in the few instances to the contrary, within limits less than the very trifling irregularities of the outlines themselves.

The graphical construction of these curves is rendered extremely easy by the elegant and well-known property of the lemniscate, in which the rectangle under two lines drawn from the foci (or poles) to any point in the periphery, is invariable throughout the whole extent of the curve. This is easily shown from its equation, and the value of this constant rectangle in any one curve is expressed by $a b$.

We must next enquire how the constant parameter b varies in passing from ring to ring. To this end I projected the rings, illuminated by red light only, on a screen as before, and having

outlined the successive loci of the minima of illumination, and laid down the poles, found the values of a & b in the several lamiscates, as in the following table :

Order of the minimum.	Observed values of a & b in square inches.	Differences.	Values of a & b computed from formula $a \cdot b = 1.59$	Excess of computed above observed values of a & b .
0	0.00		0.00	0.00
1	1.62	1.62	1.59	-0.03
2	3.165	1.545	3.78	+0.62
3	4.69	1.525	4.77	+0.08
4	6.27	1.58	6.36	+0.09
5	7.87	1.60	7.95	+0.08
6	9.56	1.69	9.54	-0.02
7	11.09	1.53	11.13	+0.04
8	12.77	1.68	12.72	-0.05
9	14.33	1.56	14.31	-0.02
10	15.98	1.60	15.90	-0.08
Mean 1.59				

The nature of the illumination not allowing the delineation to be performed with the same freedom and precision as in a fuller light, the values of a & b in the second column are the means of a great number of measures, taken in every part of their respective curves. The numbers in the fifth column exhibit the excesses of the terms of the arithmetical progression in the fourth (whose common difference is 1.59, the mean of all the differences in the third column) above the observed values of a & b , and are so small as fully to authorize the conclusion, that these values, and of course those of the parameter b , increase in arithmetical progression with the order of the rings; or, in other words, that the number of periods performed in a given space ($= 1$) by a luminous molecule going to form any point M in the projection of any ring is proportional to the rectangle of the distances P, M; P', M, of that point from the two poles.

Now, if we extend our views to crystals in which the distance between the axes is considerable, we may reasonably expect that the usual transition which takes place in analytical formulæ from the arc to its sine, when we pass from a plane to a spherical surface, will hold good. If this be the case, we shall have at once, and in all cases,

putting λ for $\frac{1}{n}$. If the plate be cut at right angles to the optic axis

and consequently

$$\sin. \theta \cdot \sin. \theta' = \frac{n}{2Kt \cdot \cos. a} (\cos. \theta + \cos. \theta'); \quad (5)$$

To put this to the trial, I took a plate of mica, whose thickness measured 0.023078 inch, and having adjusted it accurately on a divided apparatus, placed it in an azimuth 45° , and, by the intervention of the red glass above-mentioned, observed the maxima and minima of the extraordinary pencil between the poles. As these observations, when repeated, seldom agreed unless within a few minutes, 10 were taken of each maximum and minimum. The angles of incidence, deduced from a mean of similar observations on each side of the perpendicular, are set down in the second column of the following table, each number in which is, therefore, a mean result of 20 observations. The first column contains the values of n , or the order of the ring observed; the third, the angles of refraction, to obtain which I used the index 1.500, employed by M. Biot.* The fourth and fifth columns contain the values of θ , θ' , thence computed, and the 6th, values

of the coefficient h , deduced from the formula $h = \frac{\sin. \theta \cdot \sin. \theta'}{n \cdot \cos. \phi}$

TABLE X.—*Mica. Thickness = 0.023078 in.*

Values of n	Angles of incidence.	Angles of refraction.	Values of θ .	Values of θ' .	Values of h .	Excesses above mean.
0.0	55° 3' 30"	22° 31' 0"	0° 0' 0"	15° 2' 0"	0.032932	+0.000185
1.0	32 53 29	21 14 46	1 16 29	13 45 40	0.033622	+0.000475
2.0	30 34 40	19 49 30	2 41 30	12 20 30	0.033622	+0.000475
3.0	28 15 40	18 24 0	4 7 0	10 55 0	0.033095	-0.000112
4.0	26 34 20	16 43 50	5 47 50	9 14 50	0.033327	+0.000180
5.0	24 46 20	14 57 13	7 33 45	8 28 15	0.032143	+0.000091
6.0	19 35 40	12 55 10	9 35 30	5 36 10	0.033058	-0.000089
7.0	15 48 40	10 27 50	12 9 10	3 58 50	0.033026	-0.000121
8.0	11 28 50	7 11 10	15 19 50	2 42 10	0.033010	-0.000137
9.0	7 37 50	4 50 0	18 10 0	1 25 0	0.033147	

The last column of this table exhibits the deviations in excess or defect of the values of the quantity h , so computed from the mean of all of them. Their smallness, in comparison with the quantity itself, and their alternations of sign, are evident proofs

* Recherches sur les Mouvements des Molecules de la Lumière, &c. p. 482. He takes it equal to that of glass: "ce qui ne doit pas être fort éloigné de la vérité." I have attempted, without success, to measure its value. What has satisfied M. Biot and Dr. Brewster (for the latter has evidently used this index, or one very near it, Phil. Trans. 1809, p. 230) ought to satisfy every one; yet it is fortunate that in the present instance, a slight variation in the refractive index will produce but a very trifling change in the relative values of h .

of the constancy of this coefficient, and we are, therefore, authorized to take $\sin. \theta \times \sin. \theta'$ as the general value of $\downarrow(\theta, \theta')$. The observations on Rochelle salt, presently to be noticed, confirm this law.* If we denote by l the minimum length of a double oscillation, or the space passed over during one complete period by a ray transmitted at right angles to both axes, we have $l = \frac{2}{\delta}$; and consequently $h = \frac{l}{2}$, $l = h t$. If we substitute for h and δ their values above found, we obtain

$$l = 0.00076497 \text{ inch}$$

for the minimum length of a period performed by a mean red ray in mica.

Resuming our general equations (b) and (d) if we substitute the value now determined for \downarrow , and write $\frac{l}{2}$ for $\frac{k'}{k}$, we have

$$l' \cdot \cos. \phi' \cdot \sin. \theta \cdot \sin. \theta' = l \cdot \cos. \phi \cdot \sin. (\theta - \delta a + \delta \phi),$$

$$\sin. (\theta' + \delta a + \delta \phi); \quad (6)$$

whence it is easy to derive (independent of any approximation)

$$\cos. 2(a + \delta a) = \cos. 2\phi + 2 \frac{l'}{l} \frac{\cos. \phi'}{\cos. \phi} \cdot \sin. \theta \cdot \sin. \theta'; \quad (7)$$

while our approximate equation (d) furnishes the following very convenient formula for incidences nearly perpendicular

$$\sin. \delta a = \frac{l - l'}{l} \frac{\sin. \theta \cdot \sin. \theta'}{\sin. 2a} \quad (8)$$

The simplest supposition we can frame relative to the values of the constant elements l, l' is their proportionality to those of c, c' , or the lengths of the fits of easy reflection and transmission. This cannot certainly be far from the truth in crystals with one axis; in which the coincidence of the tints, with those of Newton's scale, is for the most part exact. In sulphate of lime too, and mica, the only crystals with two axes which have been examined with sufficient exactness, and under the proper circum-

* When $\theta = \theta'$, as in crystals with one axis, we have $\downarrow(\theta, \theta') = \sin. \theta$, a result long since confirmed by the accurate experiments of Brewster and Biot. The velocity of the extraordinary ray in such crystals is given by the formula $v^2 = V^2 + \alpha \cdot \sin. \theta^2$. Following this analogy, we may conclude that in crystals with two axes we should have $v^2 = V^2 + \alpha \cdot \sin. \theta \cdot \sin. \theta'$. Now this is precisely the expression at which M. Biot has recently arrived. This very simple and elegant result was communicated to me by that eminent philosopher in the spring of this year, and subsequently in a letter of May 2. His memoir on the subject which appears (by the Ann. de Chim.) to have been read to the Institute in April, I have not seen, nor do I know by what precise steps he was led to it, but presume it must have been by some considerations of the nature above described. In the foregoing investigation of the law of periodicity, I beg leave, therefore, to disclaim all intention of arrogating to myself any share in this beautiful discovery, but have thought it necessary to state the steps in the text, in order to demonstrate a truth essential to the investigations that follow, which could not have been taken for granted, or deduced by any legitimate reasoning, independent of experiment, from the equation $v^2 = V^2 + \alpha \cdot \sin. \theta \cdot \sin. \theta'$, by reason of our ignorance of the nature and mode of action of the polarising forces; and have purposely abstained from entering any further into the general laws of double refraction and polarization than I could possibly avoid.

affords for ascertaining this important point, the law of proportionality seems to be sustained with great precision. This may seem to authorize the general conclusion, that in all cases, $\frac{a}{c} = \frac{a'}{c'}$. Let us see how this agrees with the measures given in the former part of this paper.

In sulphate of baryta, if we take Dr. Brewster's measure of the dispersive power,* we have $\delta R = 0.019$, and consequently, calculating on the data determined in p. 161, we must have, at the virtual pole,

$$\varphi = 21^{\circ} 5' 30'' \quad \varphi' = 20^{\circ} 50' 30'' \quad \delta \varphi = - 15'$$

Now, if we suppose $l = 6.3463 l' = 3.9982$, the values of c and c' respectively for the extreme red and violet rays,† we shall find by substitution in our formula (h)

$$\delta a = 51' 10''$$

But a red ray penetrating the surface from within the crystal at an angle $a = 18^{\circ} 12' 30''$, and a violet one at an angle $a + \delta a = 19^{\circ} 3' 40''$, would emerge at the respective angles $30^{\circ} 59'$ and $32^{\circ} 58' 20''$, and would include between them an angle of $1^{\circ} 59' 20''$; which should be the apparent separation of the red and violet axes in the plate employed. Now, previous to the computation of this result, I had carefully measured this angle, by observing the incidences at which the extreme red and violet rays of the prismatic spectrum, received on the reflector of a graduated apparatus, respectively disappeared from the extraordinary image at the poles P, P'. I thus found

Interval of the poles P, P' for red rays $62^{\circ} 2'$

Ditto for violet. $66 \quad 5$

Semi-difference, or apparent separation of the axes, $2^{\circ} 1' 30''$, which differs from its computed value only by $2' 10''$. We may, therefore, fairly conclude, that in the case of sulphate of baryta, the hypothesis $\frac{l}{c} = \frac{l'}{c'}$ does not deviate sensibly from the truth.

If we apply our formula (f) to the measures above given for Rochelle salt, the result will be widely different. The same supposition as to the values of l , l' being made, we get

$$\delta a = 4^{\circ} 2' 50''$$

The incidence being nearly perpendicular, and the angle small, we need only increase it in the proportion $1.499 : 1$, to have the apparent angle, which thus comes out $6^{\circ} 4'$. We have already found $9^{\circ} 46'$ for the same angle, by a method which must necessarily give a result much below the truth. This difference is by far too great to arise from any errors of observation; but, to obtain more exact measures, I took several times the apparent

* $\delta R = 0.019$. *Treatise on New Philosophical Instruments.*

† Biot, *Traité de Physique*, vol. iv.

angular separation of the axis of each colour from that of the extreme red by the direct homogeneous light of a sunbeam, separated by the prism, and received on the reflector of a divided apparatus, when, after the proper reductions for refraction and dispersion, the results were as follow:

Colour.	Apparent separation of the axes.	Real separation — δ, ϵ —	Values of $2 a_1$	No. of observations.
Extreme red	0° 0'		75° 42'	18
Mean red	1 33	1 2'	73 38	45
Mean orange	2 37	1 44	72 14	18
Mean yellow	4 0	2 40	70 23	20
Mean green	4 48	3 32	67 57	16
Mean blue	8 2	5 21	65 0	18
Mean indigo	10 21	6 54	61 54	33
Indigo violet	11 17	7 31	60 40	2
Mean violet	13 54	9 17	57 8	2
Extreme violet	15 23	10 14	55 14	8

Though the total separation of the red and violet axes in this table so far exceeds what we had before estimated it at, I am fully satisfied that it is no way exaggerated, but rather falls short of the truth. It is very practicable, by combinations of coloured glasses, liquids, &c. to insulate either extremity of the spectrum in a state of the most absolute purity. In this climate, the dispersed light of the sky in the neighbourhood of the sun, which always mixes with the prismatic beam, is so considerable as to obliterate the feeble rays which compose the two extremities of the spectrum, and it is only by interposing such combinations between the eye and the Iceland crystal used to analyze the polarised ray, that they can be examined with any certainty. The combination I employed for the extreme red was such that when the whole spectrum thrown on a white screen was viewed through it, it was seen reduced to a perfectly circular, well defined, deep red image, whose centre fell on the very furthest termination of the red as seen by the naked eye, and whose circumference attained, or perhaps surpassed the point where the maximum of the calorific rays has been supposed to be situated. In like manner, when the same spectrum was examined with the violet combination, a very slightly elongated violet image became perceptible, but every trace of the indigo, and the brighter portion of the violet rays, was extinguished. For observations on the indigo, and all the more refrangible portion, I employed similar artifices, without which I found it perfectly impracticable to obtain any regular and comparable results.

The coefficient $\frac{l-l'}{l}$ in our formula being the only part not immediately deduced from observation, it is evident that the assumption $\frac{l}{l'} = \frac{c}{c'}$ must be widely erroneous in the present

instance, and it, therefore, becomes necessary to ascertain the values of l by direct measures. This is rendered easy by the equation (c) which gives

$$l = \frac{\sin. \theta \cdot \sin. \theta'}{\sin. \phi}$$

We have only, therefore, to observe the inclinations of a plate of known thickness, properly cut and adjusted to 45° azimuth, which correspond to the alternate disappearances of the ordinary and extraordinary images, at which points the values of ϕ are $\frac{1}{2}, \frac{3}{2}, \frac{5}{2}, \frac{7}{2}, \&c.$; computing then the values of θ, θ' , and ϕ , and substituting, we get the values of l , without detailing particular experiments. The following table expresses the final result of a great number of such measures;

Colour.	Values of l in inches.	Number of observations.
Extreme red	0.0026158	64
Mean red	0.0050032	14
Mean orange	0.0045852	24
Mean yellow	0.0040583	32
Mean green	0.0036549	62
Mean blue	0.0032863	22
Mean indigo	0.0029868	52
Extreme violet	0.0025693	49

The observations from which this table was calculated were made indiscriminately on the maxima and minima of all orders. Those of different orders were of course computed separately, and found to agree without exception in giving the same values of l within limits of error less than those to which the observations are liable; thus affording another proof of the exactness of the law of periodicity above employed. Now, if we compare these one with another, and with those of c as deduced by M. Biot from Newton's observations, we shall have as follows:

Colour.	Values of $\frac{p}{l}$	Values of $\frac{d}{c}$
Extreme red	1.00000	1.00000
Mean red	0.39993	0.96215
Mean orange	0.81659	0.90490
Mean yellow	0.72266	0.85550
Mean green	0.65082	0.79433
Mean blue	0.58530	0.73725
Mean indigo	0.53154	0.68641
Extreme violet	0.44684	0.63000

It appears from this comparative statement, that the forces of polarisation and double refraction in the body now examined, act with much greater proportional energy on the more refrangible rays than in mica, sulphate of lime, and other similar bodies,

and consequently that, even were its axes coincident, its tints, though perfectly regular, would still differ very sensibly from the colours of thin plates. This secondary cause of deviation ought to become sensible in plates cut so as to contain both axes, if examined at a perpendicular incidence; but, I have not yet had an opportunity of making the trial.

If we calculate on the numbers above given, it will soon appear that a perfect coincidence of all the colours in a single virtual pole is impossible. For this purpose we may employ our equation (7) which easily affords the following

$$\cos. 2(a + \theta) = \cos. 2a \left\{ 1 + \frac{2l}{l-l'} \cdot \tan. 2a \cdot \sin. (-\delta a) \right\} \\ = \frac{\cos. 2a}{(\cos. M)}$$

taking M an auxiliary angle such that

$$\tan. M = \sqrt{2 \cdot \tan. 2a \cdot \frac{l}{l-l'} \cdot \sin. (-\delta a)}$$

whence the value of θ or the position of the coincidence of any two coloured rays becomes known, the values of l , l' , and $-\delta a$ being given from the foregoing tables. If we unite the mean red with the mean green, these formulae give $\theta = -11^\circ 29'$, and if with the mean blue, $\theta = -14^\circ 8'$, of which the one falls short of, and the other exceeds the angle $-13^\circ 1'$ given by observation. If we determine by interpolation the values of l' and $-\delta a$, which give $\theta = -13^\circ 1'$, we shall find very nearly

$$l' = 34581 \quad -\delta a = 3^\circ 37' \quad -\delta a + 1^\circ 2' = 4^\circ 39'$$

which correspond to a blue ray strongly inclining to green, and in the brightest part of the colour. Now it is evident that when a rigorous union of all the rays in the proportion in which they exist in white light, is impossible, that of the strongest and brightest colours in opposition to each other will at least ensure the nearest approach to a virtual pole on the principles above demonstrated, and a white will thus be produced, not indeed mathematically perfect, but containing no marked excess of any of the more powerful colours.

The apophyllite is the only crystal with one axis whose tints exhibit a sensible deviation from the scale of Newton. Its phenomena, however, are entirely independent of the first and principal cause which produces the deviation in crystals with two axes, viz. the separation of the axes of differently coloured rays, and are referable solely to the secondary and subordinate cause, of which Rochelle salt has just afforded an example, viz. a peculiarity in the law which regulates the lengths of the minimum oscillations of the differently coloured rays within the medium.

1. The tints of the apophyllite commence at the centre of the rings, and increase in regular progression outwards, following the same order, whatever be the thickness of the plate.

It follows from this, that the multiplier M in our general for-

mula (α) is the same for all the coloured rays, being zero at the commencement of the scale; and hence it follows, as a necessary consequence, that the axes of all the colours are united in one, and the virtual and actual poles coincide with each other and with the centre. Did any sensible separation of the axes exist, it must become perceptible by the ellipticity of the rings when examined with homogeneous light of that colour from which they are furthest asunder; but with the greatest attention, in plates of considerable thickness, I have not been able to observe the slightest shifting of the axis, or deviation from the circular figure, in passing from a red to a violet illumination. Moreover, it is evident from the preceding theory, that any difference which may exist in their position, if too small to be sensible to the eye, can produce only an imperceptible deviation of tints. In fact, if we suppose $\alpha = 0$ for any colour, we get, for the position of the virtual pole,

$$\sin. \theta = \sqrt{\frac{1-\beta}{\gamma}} \cdot \sin. \delta \alpha$$

θ being the angular distance of the point of coincidence from the single axis of that colour. It is, consequently, insensible when $\delta \alpha$ is so. Now, the polarising force of the apophyllite being very feeble, the diameters of the rings in any plate of moderate thickness must so far exceed this very minute quantity that the virtual poles, did any exist, must fall within the limit of the central blackness; the Newtonian scale would still appear to commence from the centre, nor could any sensible deviation from it arise from this cause.

2. When the prismatic spectrum is passed over an apparatus containing a plate of this mineral, no perceptible change in the *magnitudes* of the rings for different colours takes place. Hence it appears that the value of the function l for all the coloured rays is nearly alike. By measures taken on a divided apparatus, a slight difference is observed. Taking the mean refractive index R at 1.5431 (by a very careful measure) and the dispersion δR at 0.017, the formula

$$l = t \cdot \frac{\sin. \phi^2}{n \cdot \cos. \phi} = \frac{t}{n} \cdot \sin. \theta \cdot \tan. \theta$$

gave as follows:

Extrema ved.....	$l = 0.002066$
Mean red.....	0.0022810
Mean orange.....	0.002337
Mean yellow.....	0.002483
Mean green.....	0.002646
Mean blue.....	0.0028059
Mean indigo.....	0.002964
Extreme violet.....	0.010660?

This table, though not given as exact, owing to imperfections in the specimen examined, agrees with the succession of tints, which, as far as the fourth order, were as follows:

Apophyllite. Thickness = 0.0629 inch.

Incidence.	Ordinary pencil.	Extraordinary.	
0° 0'	Bright white	Black	The axis
13 50	White, with a trace of purple	White, slightly greenish	
21 50	Exceeding sombre violet ..	Pure bright white	1st max. of illumination
25 12	Pale greenish yellow	Purplish white	
29 45	White	Sombre violet blue	
30 50	White	Extremely sombre violet	1st minimum
33 3	White, with a strong tinge of violet	Pale yellow green	
35 50	Blue, strongly inclining to purple	Greenish white	
37 20	Sombre indigo, inclining to violet	White	
38 5	Sombre violet	White	2d maximum
40 10	Tolerably good yellow green	Purplish white	
43 55	White, with a trace of yellow	Obscure indigo, inclining to purple	
44 27	White	Sombre violet	2d minimum
46 45	Pale purple	Tolerably yellow green	
49 57	Sombre purple blue	Yellowish white	
50 30	Sombre violet	White	3d maximum
53 40	Green yellow	Pale purple	
56 0	Yellowish white	Sombre indigo blue	3d minimum
56 40	Yellowish white	Sombre violet	
	White	Livid grey	
59 35	Pale purple	Yellow green	
61 45	Sombre indigo	Pale yellowish white	4th maximum
62 10	Sombre violet	Yellowish white	
63 0	Faint violet white	White	
	Livid grey	White	
66 0	Tolerably green yellow ...	Purple	
69 30	Yellow white	Very sombre indigo	4th minimum

In the colours of thin plates and others of the like composition, the difference in the lengths of the periods of the different rays is so considerable, that after seven or eight alternations of colour, the rings confound one another, and are blended into a uniform whiteness. Were the periods more nearly equal, a greater number of rings should be visible, and were they strictly so, the succession of alternate whiteness and blackness should be continued to infinity. As the values of l , l' , &c. in the apophyllite approximate pretty closely to this limit, we should expect to see a much greater number of rings, and this I find to be really the case. By inclosing a thick plate in balsam of copaiba in a proper apparatus to increase the range of incidence, I have counted as far as the 35th order, when I desisted; not from any want of alternate colours, but owing to their extreme closeness, which rendered it impossible to number them distinctly. Indeed I have no doubt that could a very thick and limpid specimen be procured, hundreds might be seen without artificial aid.

In two instances then, at least, and probably in many more,

or perhaps to a certain small degree in all cases, the minimum lengths of the periods deviate in their respective proportions from those of the fits of easy transmission and reflection; a circumstance which of itself is sufficient to prove the independence of the causes of these laws of periodicity. If we take $Rr = RA$, fig. 5, and construct a curve whose abscissas AP are the values of $a, a', &c.$ and ordinates those of $l, l', &c.$ the straight line $ro y g b i v$ inclined at 45° to AR will represent the locus for crystals, such as carbonate of lime, in which the periods follow the Newtonian law, $r' o' y' g' b' i' v'$ will represent the same locus for tartrate of soda and potash, while $r'' o'' y'' g'' b'' i'' v''$ is the curve similarly traced for apophyllite.

Having communicated to Dr. Brewster my observations on the deviation of tints, and the conclusion I had thence deduced as to the separation of the axes of the differently coloured rays, I received in answer a letter, from which, in justice to that indefatigable observer, I subjoin the following extracts:

MY DEAR SIR, *Esk Hill, by Roslin Larwade, Sept. 18, 1819.*

"In consequence of having been some time from home, I have only now received your letter, and hasten to reply to that part of it in which you request me to state what results I had obtained respecting the deviation of the tints from Newton's scale. The following general points will enable you to judge of the progress which I had made in the inquiry.

"1. In almost all crystals with two axes there is a deviation from the tints of Newton's scale.

"2. This deviation is greater in some crystals than in others, being a maximum in acetate of lead and tartrate of potash and soda.

"3. That all these crystals may be divided into two classes, viz. those which have the red ends of the rings inwards and the blue ends outwards, and those which have the red ends outwards and the blue ends inwards.

"4. That in all crystals with two axes, the doubly refracting force of one axis in general acts differently upon the differently coloured rays from the doubly refracting force of the second axis.

"5. That as the polarising force is always proportional to the force of double refraction, the polarising force of one axis will act differently on the differently coloured rays from the polarising force of the other axis.

"6. * * * * *

"7. The consequence of this is, that there will be different resultant axes, or different points of compensation for the differently coloured rays.

"8. All these effects may be calculated with the utmost accuracy, if the ratio of the dispersive powers of the two extraordinary

refractive forces is given, or *vice versé*, the dispersive powers may be obtained from the angles of the resultant axes for the red and violet rays of the spectrum.

" 9. I have found crystals in which these phenomena are *decidedly connected* with the rotatory phenomena; and from this highly important fact I am led to conclude that both have the same origin, and that all the rotatory phenomena are, as I have stated in my paper, the result of the uncompensated tints of two axes, equal for the mean ray, but unequal for all the rest. (*Here follows an illustration by a diaphragm.*)

" 10. The division into two classes in sect. iii. as founded merely on observation, is converted into another division into two classes; viz. 1. That in which the doubly refracting force of the principal axis acts more powerfully on the blue rays than the other axis does; and 2. That in which it acts less powerfully. The first class comprehends those crystals in which the blue ends are inwards, and the second, those in which the red ends are inwards, or nearer the principal axis."

In a subsequent letter (Oct. 4), he adds,

" The *virtual poles*, which you mention, I discovered in the year 1816, and I have two accounts of them in my Journal, the one signed on the 24th January, 1816, and the other 6th January, 1817, by Sir G. Mackenzie, President of the Physical Class of the Royal Society."

No comments on the above extracts are necessary. They establish at once the priority of Dr. Brewster's observations, and the independence of mine. With regard to the division of crystals into two classes, which observation has alike suggested to both of us, it is unnecessary, if we regard either of the two classes as having the angle between the resultant axes greater than a right angle. In Dr. Brewster's table, Phil. Trans. 1818, p. 230, succinic acid and sulphate of iron are stated as having this angle 90° . If this determination corresponds, as in all probability it does, to the yellow rays, they belong at once to both classes, and are, in fact, instances of the limit where one class passes into the other. Bicarbonate of ammonia, in which I can perceive no separation of the axes of different colours, nor of course, any virtual poles, belongs in like manner to both classes, or to neither.

JOHN F. W. HERSCHEL.

APPENDIX.

Description of an Instrument employed in the foregoing Experiments on the polarised Rings.

The singular property possessed by the tourmaline, by which a plate of it of any moderate thickness cut in a direction parallel to its axis of double refraction, is enabled to absorb the whole, or

nearly the whole, of an incident pencil polarised in a plane parallel to that axis,* was pointed out by M. Biot, in the fourth volume of his *Traité de Physique*, and he has availed himself of it with his accustomed ingenuity, as affording an extremely ready and convenient mode of viewing the phenomena of polarisation, much more so than by the use of plates of agate, prisms of Iceland spar, or a second reflection. It follows, from the above-mentioned property, that if a beam of ordinary light be made to traverse such a plate, the whole of the emergent pencil, or nearly so, will be polarised in a plane at right angles to the axis; for the incident ray being divided by the doubly refracting force into two pencils, polarised in planes, the one parallel, the other perpendicular to the axis, the former is extinguished in its passage, while the latter emerges with nearly its full intensity.

Hence, if two such plates are crossed at right angles, though separately very transparent, their combination will be opaque. There is a great difference, however, in the degree in which tourmalines of different colours possess this power. Those of a light green, pink, or bluish colour, are quite improper, allowing a considerable portion of light to pass when so crossed; while, on the other hand, those whose colour verges strongly to the honey yellow, or to the hair brown, or purplish brown, effect nearly a complete absorption, and afford, when crossed, a combination almost impervious to light. In ignorance of this distinction, I sacrificed several fine and valuable specimens before I could obtain proper plates.

When a crystallized lamina, cut in a proper direction, is interposed between such a combination of plates, it disturbs the polarisation which the light has received in traversing the first plate, and renders a certain portion of it capable of traversing the second: the colour and intensity of this portion, varying with the direction of the ray, give rise to the phenomena of the polarised rings, which may accordingly be seen by applying the eye, and receiving on it the dispersed light of the clouds, &c.

In order, however, to equalize as well as disperse the light, which is of great importance to obtaining a perfect view of the phenomena, an emerald glass may be cemented on the anterior plate, or the first surface of the plate itself roughened; but it will be found more convenient in practice to employ a double convex lens of short focus for this purpose, by which, if necessary, a very strong illumination may be obtained, and an extremely minute portion of a crystal subjected to examination.

* The same property is observable in the epidote, the axinite, and all other natural and artificial crystals which exhibit any degree of dichroism when examined by unpolarised light. Muriate of palladium and potash possesses it in the highest perfection. This remarkable effect is easily explained by a reference to the general principles laid down by Dr. Brewster in his paper on absorption, *Phil. Trans.* 1819, p. 11. The incident pencil is separated by the doubly refractive force into two, oppositely polarised, one of which is partly absorbed, the other emerges (polarised in its proper plane) of nearly its original intensity.

I have thus occasionally examined the rings in a portion not exceeding the hundredth of an inch in diameter, and thus detected irregularities of crystallization of a very singular nature, in many bodies, which would have eluded any other mode of observation. For this purpose the crystal must be cemented over a small aperture in a thin sheet of brass, on which the focus of the lens must be exactly adjusted to fall.*

If, instead of applying the eye to receive the light so dispersed, we place a screen at some distance in a darkened room, the apparatus is converted into a solar microscope, and the rings will be seen projected on the screen. The construction of the apparatus I employed is as follows: A B is a brass tube, within which are fitted, first, a fixed diaphragm, *a a b b*, carrying the first plate of tourmaline in its centre; secondly, a diaphragm, *c c d d*, moveable freely in its own plane by means of the pin *g* passing through a slit in the side of the cylinder, A B, which occupies an arc of about 120° of its circumference. This is destined to receive the crystallized plate, *d d*, while a cylinder, *h h e e f f*, made to slide and turn smoothly within A B carries the second tourmaline, *f f*. It is essential that the tourmalines employed for this purpose, and especially the posterior one, should be perfectly free from all flaws and blemishes; but large plates not being required, this condition is easily satisfied. The plates so arranged, and brought as near together as possible, the extremity A of the cylinder A B is fitted to slide somewhat stiffly on the brass tube P Q, furnished with a lens L, of about two inches focus, and a screw P P, by which it can be adapted to the apparatus usually employed for reflecting a sunbeam into a darkened chamber. The sliding motion of the cylinder A B allows the focus of the lens to be adjusted so as to fall exactly on the first surface of the posterior tourmaline *f*, while its rotation suffers the axis of the anterior one to be placed perpendicular to the plane of reflection. By this arrangement two advantages are gained. The reflector employed (though metallic) always polarises a more or less considerable portion of the reflected beam, which in any other position is partially or totally extinguished by the first tourmaline, and a great loss of light ensues, which it is of the utmost consequence to avoid: moreover, by this disposition, the action of the reflector is brought to conspire with that of the tourmaline, and the polarisation of the light which traverses it (which is never rigorously exact) is thereby rendered more complete.

It is convenient to have sliding tubes containing lenses of different focal lengths, according to the crystal examined, for the

* I have now an apparatus preparing, in which the first plate of tourmaline itself is formed into a double convex lens, by which the loss of light at two surfaces will be suppressed. It is easy to adapt such a lens to a double microscope, for the purpose of detecting microscopic irregularities; and I have reason to suppose a variety of curious results will be brought to light by these means.

intensity of illumination any point in the screen being, *ceteris paribus*, as the square of the focal length; consequently, when the rings lie within a very small angular compass, a greater illumination of every part of them may be obtained by using a lens of a longer focus.

The dimensions of the figure, fig. 6, are nearly of the actual size.

ARTICLE II.

Observations on a Memoir "On the Theory of Franklin, according to which Electrical Phenomena are explained by a single Fluid," read at the Royal Institution of the Sciences at Amsterdam, by M. Martin Van Marum, Knight of the Order of the Belgic Lion, Secretary of the Dutch Scientific Society, Director of the Teylerian Museum, &c. &c. (With a Plate.)*

(To the Editor of the *Annals of Philosophy*.)

SIR,

Dec. 13, 1820.

It has been repeated so often as almost to require an apology for its introduction, that the Baconian philosophy proceeds by discovering and establishing facts, holding that upon such a foundation alone, can be raised any structure deserving the name of science. The philosophy which has happily been almost exploded by Bacon and his successors, lays its foundation in hypotheses; and the labours of its adherents are spent in ingenious conjectures, or in efforts to bend the various facts discovered, to give those conjectures an apparent truth. A Baconian philosopher gathers his maxims and principles from the united rays of numerous observations and experiments, and as they are received in all the simplicity with which the facts themselves express them, they yield to the mind all that satisfaction and confidence which spring from the clear perception of truth. On the contrary, an hypothesis is incapable of teaching any truth at all, and ought, under no circumstances, to be received as "a confirmed truth;" the observation being certainly well-founded, "that an hypothesis, however satisfactory as far as concerns the explanation of all the phenomena for which it has been proposed, cannot nevertheless, for this reason, be considered as incontestably proved."†

Unfortunately the worthlessness of hypotheses is not their greatest evil; they invariably tend either to mislead the mind from those conclusions naturally deducible from experiments, or

* *Ann. Phil.* No. 96, p. 440.

† *Ann. Phil.* No. 96, p. 441. I conceive this must be the meaning of the sentence which it is apprehended (though without an examination of the original paper), must be mistranslated.

to induce an extravagant regard to some circumstances to the neglect of others equally or more important. Thus Franklin, whose experiments and observations will ever rank him among the first philosophers, has impeded the progress of science by his hypothesis respecting electricity, which now, in spite of the anxious exertions of his old and most respectable disciples, seems inevitably doomed to death. M. Van Marum however is of a different opinion, and to arrest the progress of the dualists, by which he means those who think there are two electric fluids, by the assistance of the Royal Institution of the Sciences at Amsterdam, he has thrown down the gauntlet to all the world, in a paper read before that Institution in October, 1819, and in their name he has called for an answer to an experiment which he originally published in the year 1785, and considers still unanswered and unanswerable.

The experiment which has been thus put forward as an experimentum crucis of the Franklinian hypothesis, is given so little in detail, and so many of the important circumstances are omitted, that it is difficult to deduce from it a perfect explanation of the phenomena. From the whole of the statement, however, the facts upon which M. Van Marum relies may perhaps be sufficiently collected. It seems then, that in the formation and collection of positive electricity, M. Van Marum employed two conductors; one in more immediate connexion with the machine, which he has called the first conductor, and a second intended to contain a larger quantity of electricity, which he calls the receiving conductor, and this latter was placed at a little distance from the first conductor. It then appears, that he was able to ascertain by the form of the spark, that the electric fluid which was generated by the machine, and communicated to the first conductor, passed from that conductor to the receiving conductor.

Perhaps there cannot be shown in philosophical reasoning a more flagrant instance of the evil of an hypothesis and its pernicious influence over the mind, than that M. Van Marum and his friends should esteem this "a most evident proof in favour of the theory of Franklin." That is, because one electric fluid can pass from one conductor to another, therefore there is no other electric fluid.

This experiment could satisfy only the most zealous adherents of the Franklinian hypothesis; but another, which is twice detailed, is considered irresistible. It was so powerfully operative upon a French philosopher as to have silenced him for ten years.*

The material circumstances of this experiment can hardly be gathered from either statement of it, but by comparing the two accounts together, the more important facts may probably be

* *Ann. Phil.* No. 96, p. 447.

distinguished. From the comparison of the two statements, it appears that if "a *button* of copper*" which either was fixed in a conductor charged by the machine with positive electricity, or by communicating with the earth, acquired a charge of an opposite kind to that of the electrified conductor "be brought within six inches of the *large globe*"* of the conductor negatively electrified, the ramified electric fluid passes from the positive to the negative conductor. This last experiment in M. Van Marum's hands has proved victorious over every person with whom it has come in contact; and it is not, therefore, surprising that it should have convinced even the venerable, sober-minded, and intelligent Franklin of the truth of *his own hypothesis*.

Happily, however, with whatever genius an hypothesis may be first suggested, or however ably and inflexibly supported, truth will ultimately become rebellious. Just as in this case, where the similarity of the effects of positive and negative electricity is so obvious in innumerable instances that it has been impossible to repress the opinion that if the phenomena of positive electricity arise from a peculiar fluid, so do those of negative electricity. While a philosopher whose mind is accustomed to inductive reasoning will not be able to avoid some surprise that such a man as Franklin, on seeing M. Van Marum's experiment, should say, "this *proves* the theory of a simple electric fluid, and it is now high time to reject the theory of two sorts of fluids." To such a philosopher, it must appear singular that it should not have suggested itself to Franklin, that it might have been, that the negative electricity had arisen from a peculiar fluid, which was held in its conductor by a stronger attraction than that by which the positive electricity was held in its conductor; or it might have been that the air (which probably occasions the zigzag ramified appearance of the positive spark), opposed less resistance to the positive fluid than to the negative; or it might have been that the form of the button afforded greater facility to the transmission of an electric fluid than that of a large globe; and either of these circumstances would account for the passage of the electric fluid from the positive to the negative conductor, consistently with the notion of a negative electric fluid. But if every one of these circumstances were shown not to exist, the experiment could not possibly *prove* that there was no negative or resinous electric fluid. Even then the fact that the positive electric fluid had been attracted to the negative conductor, would no more have *proved* the non-existence of a negative or resinous electric fluid, than the fact that concentrated sulphuric acid attracting water to itself proves the non-existence of sulphuric acid.

But M. Van Marum's experiment will be better explained, and his observations answered, by another experiment, which

was published in the *Philosophical Transactions* so long since as the year 1789.

"The escape," says Mr. Nicholson, in a paper detailing some experiments with a powerful machine, "of negative electricity from a ball, is attended with the appearance of straight sharp sparks with a hoarse or chirping noise. When the ball was less than two inches in diameter it was usually covered with short flames of this kind, which were very numerous.

"When two equal balls were presented to each other, and one of them was rendered strongly positive, while the other remained in connexion with the earth, the positive brush or ramified spark was seen to pass from the electrified ball: when the other ball was electrified negatively, and the ball, which before had been positive, was connected with the ground, the electricity exhibited the negative flame, or dense, straight, and more luminous spark, from the negative ball; and when the one ball was electrified plus and the other minus, the signs of both electricities appeared. If the interval was not too great, the long zigzag spark of the plus ball struck to the straight flame of the minus ball, usually at the distance of about one-third of the length of the latter from its point, rendering the other two-thirds very bright. Sometimes, however, the positive spark struck the ball at a distance from the negative flame. These effects are represented in Plate V. figs. 1, 2, and 3.

Two conductors of three-quarters of an inch diameter, with spherical ends of the same diameter, were laid parallel to each other, at the distance of about two inches, in such a manner as that the ends pointed in opposite directions, and were six or eight inches asunder. These, which may be distinguished by the letters P and M, were successively electrified as the balls were in the last paragraph. When one conductor P was positive, fig. 5, it exhibited the spark of that electricity at its extremity, and struck the side of the other conductor M. When the last-mentioned conductor M was electrified negatively, fig. 4, the former being in its turn connected with the earth, the sparks ceased to strike as before, and the extremity of the electrified conductor M exhibited negative signs, and struck the side of the other conductor. And when one conductor was electrified plus and the other minus, fig. 6, both signs appeared at the same time, and continual streams of electricity passed between the extremities of each conductor to the side of the other conductor opposed to it." "The effect of a positive surface appears to extend further than that of a negative."

This experiment will need few comments, for it is sufficiently evident that if the form of a spark or its direction, is to determine the existence of an electric fluid, this experiment proves that there are two. Nor is it easy for an unprejudiced observer

Fig. 1.



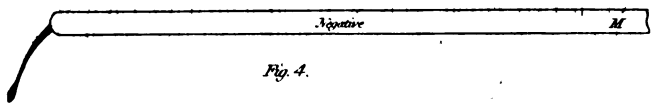
Fig. 2.



Fig. 3.



Fig. 4.



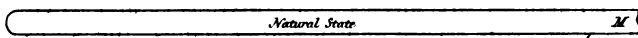
Natural State.

P

Natural State.

M

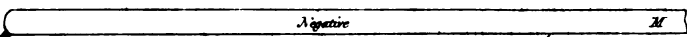
Fig. 5.



Positive

P

Fig. 6.



Positive

P

Fig. 7.

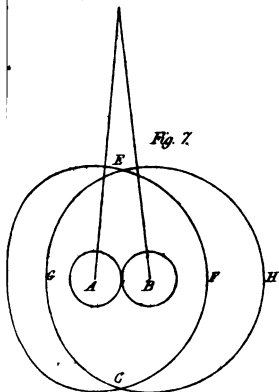


Fig. 8.

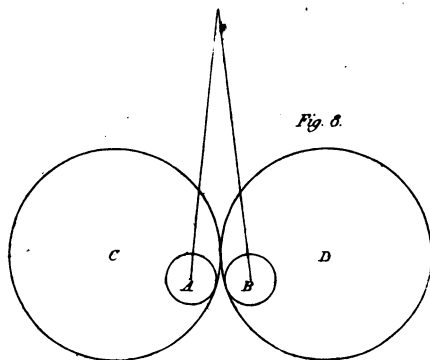
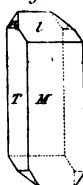


Fig. 9.



Fig. 10.



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Fig. 11.

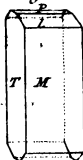
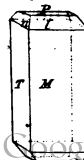


Fig. 12.



to explain, how able, intelligent philosophers should be so spell-bound by an hypothesis, as, in defiance of the plainest observations of their senses, to assume, that in all these instances, the electric spark was just the same, produced by one and the same cause, a single electric fluid, passing in every case from the plus, or positive, to the minus, or negative conductor.

These observations on M. Van Marum's paper would be here concluded, but there is another part of it relating to the apparent repulsion of electrified light bodies, containing a few arguments so novel and original, that though it is not intended generally to enter in this place upon the subject of electrical repulsion, it is difficult to avoid taking some little notice of them.

M. Van Marum first assumes, that it is "a sound truth, and well confirmed by experience, that no electric force can arise from the surface of a body, unless this latter be in contact with bodies either solid or fluid which assume a contrary force, or be surrounded by such bodies."

To be sure it has seemed to be placed among the elements of electrical science, confirmed by every experiment, that when bodies having opposite electricities are brought into contact, the electricities, unless controlled by superior attractions, combine and are neutralised. But it is not here intended to controvert the bases of the reasoning, but to notice the method of argument founded upon those bases.

M. Van Marum proceeds. "A body positively electrified and placed in the air is, therefore, conformably to this law surrounded with an atmosphere of contrary electric power; and a negatively electrified body is, on the other hand, surrounded by a positively electrified atmosphere." "It is evident that every electrified body must necessarily be placed *in the midst* of this atmosphere of an opposite electric force, the contrary being impossible."

Now if these propositions were true, it is quite clear that an electrified sphere would form around itself a spherical electrified atmosphere of which the sphere would be in the centre. If, therefore, two similarly electrified pith balls were suspended in contact, they would form around themselves spherical atmospheres having an opposite electric force to themselves, fig. 7. The pith-ball A would be surrounded by the atmosphere C D E F, and the pith-ball B by the atmosphere C G E H. It is evident that the centre of the joint atmospheres would be at the point of contact of A and B, to which, therefore, they would both be attracted, and consequently would remain at rest. But these atmospheres would not suit M. Van Marum's hypothesis, nor is it consistent with the facts that the pith-balls should remain at rest. These difficulties are most ingeniously overcome, by M. Van Marum's *supposing* that the pith-balls A and B, fig. 8, should form atmospheres round centres C and D at a distance, and then that each atmosphere would attract its pith-ball to that centre. Why the balls should form atmospheres round those

centres before they themselves get there, or how they should be attracted by the atmospheres before those atmospheres are formed, M. Van Marum has not suggested, nor is it easy to imagine.

It is not, however, fair to complain of M. Van Marum's supporting one hypothesis by another, nor do I perceive that his own hypothesis is more contrary to his own propositions than the Franklinian hypothesis is to the analogies of nature. Franklin supposes that there is in all bodies a natural quantity of electric fluid, which cannot be perceived, and produces no effects; and which consequently is from its very nature incapable of any proof; that it so perfectly combines with bodies as entirely to lose all its properties, yet is held by so slight a force as to be removed by the least possible attraction; as in the communication of negative electricity. Such a fluid is perfectly anomalous, leaving, therefore, the hypothesis not only unsupported by fact, but even by analogy.

But it may be said, if there be two electric fluids which on combination neutralise each other, what becomes of that compound? It certainly would not follow that if no answer could be given to the question, therefore the Franklinian hypothesis must be correct. That would still be entirely an hypothesis, and, therefore, ought at once to be rejected, as offering the wrong path to truth: nor ought, on the other hand, any hypothesis to be received in reply to such a question. In order, however, that we may not be misled from the proper answer, it may be well to observe that it ought not to be expected that the compound fluid should possess the same properties as its component parts any more than water does, or nitric acid. Another question may perhaps lead us to the proper answer. Is there any thing always produced by the combination of two electricities? If there be, we are bound to assume that the produce is the compound of the two fluids. Oxygen and hydrogen combine, and the caloric being disengaged, water is produced; and we assume that water is a compound of oxygen and hydrogen. The two electric fluids combine, and caloric is produced, and that not from any concussion of the air, for it is produced in vacuo. Why then is not the same inductive reasoning to be admitted in ethereal as in gaseous fluids? Why should we not admit the suggestion which has been offered,* that caloric is a compound of the two electricities? I remain, Sir, yours, &c. C.

* *Essay on Heat, Light, and Electricity*, by C. C. Bompas.

ARTICLE III.

Remarks on Mr. Phillips's Analysis of the Pharmacopœia Collegii Regii Medicorum Edinburgensis, contained in a Letter addressed to R. Phillips, Esq. By Dr. Hope, Professor of Chemistry in the University of Edinburgh.*

SIR,

Edinburgh, Feb. 12, 1821,

THE Royal College of Physicians of Edinburgh in preparing new editions of their Pharmacopœia gladly avail themselves of every suggestion that may enable them to render that work worthy of the public confidence; and I entertain no doubt that hereafter they will continue to be thankful for services of the like description under whatever form they may present themselves.

Well acquainted with your knowledge of chemistry in general, and your attention to the chemical department of Pharmacopœias in particular, I confidently expected that the criticism which you have thought it worth while to give of the last edition, even after a lapse of nearly four years since its publication, would have afforded much useful information to guide us in preparing a future one. I cannot, however, conceal how greatly I have been disappointed in this respect. As your remarks are far from being in a strain of approbation, and as the chemical department had in a great measure been entrusted to me, I feel that I owe it to the College, over whom I had the honour of presiding, when that edition came forth, to maintain their credit in regard to this work, which, from a certain degree of national authority in the preparation of drugs attached to it in this part of the empire, ought to be as free from blemish, and stand as high in the public estimation, as possible.

I undertake the task without reluctance, both because I consider it a duty incumbent upon me, and because, if I do not deceive myself, I can easily make it appear that the strictures which you have published upon its formulas are by no means well founded.

Had you been aware that the late edition was several years under revision, and that many trials were made of the different processes directed in the most esteemed Pharmacopœias in Europe with the view of ascertaining their comparative merits, and that those adopted by us have in general been many times repeated, I am persuaded that the tone at least of the criticism would have been considerably different.

Permit me to observe that most of your objections to the formulas apply to the relative quantities of the materials employed, and rest upon these quantities deviating from the proportions of

* *Annals of Philosophy*, No. I. (New Series.)

188 *Dr. Hope's Remarks on Mr. Phillips's Analysis* [MARCH, combination stated in Dr. Wollaston's table of chemical equivalents. I apprehend, however, that you have made an application of this beautiful and valuable contrivance which its very ingenious author never contemplated, and could not now sanction; for though that table displays the proportions in which different substances combine, it by no means displays the relative quantities of the substances to be employed, when decompositions are to be effected, particularly by single affinity. It has been long known to chemists that to achieve the complete decomposition of any quantity of a compound, an excess of the decomposing material is either absolutely necessary, or very useful, by accelerating the operation.

Were I not addressing myself to a person thoroughly versed in the science of chemistry, it would be easy to explain in detail the reason of the circumstance; but it will be sufficient to remark that this excess is in some cases indispensable from the disposition of the decomposing agent to form a super or bicomponent with the ingredient to which it is to attach itself, as in the decomposition of nitrates and muriates of alkalies by sulphuric acid to be immediately brought into view; and also when the substances are in the state of dry powder, in order that each particle of the compound may fully and freely encounter those which are to act upon it.

The reader who may think it worth his while to peruse these observations is particularly requested to direct his attention to your critique commencing in p. 58 of the first number of the *Annals of Philosophy*, and to read in succession the different articles to which a reply is now to be given.

Acidum Aceticum Forte is the first substance of which you take notice, and your objection to the formula for its preparation is, that the quantities of the salts employed are not such as are required for mutual decomposition. It is unquestionably true that the sulphate of iron contains more sulphuric acid than is required to saturate the oxide of lead in the acetate, but it is of advantage to employ this excess. It facilitates greatly the disengagement of the acetic acid, and renders it unnecessary to raise the temperature to so high a pitch as would otherwise be required, by which means the empyreuma, unavoidable in an elevated temperature, is in a great measure prevented; hence at the trifling expence of an additional quantity of the sulphate of iron, and of an increased size of utensil, there is a saving of time and of fuel, and a vast gain in the quality of the product. The object of this process is to obtain a very strong acid capable of dissolving camphor at a cheaper rate than from acetate of copper.

Acidum Muriaticum.—Upon the preparation of this substance you remark, that "Equal quantities of sulphuric acid and common salt are directed to be employed in the preparation of this acid. It will be seen by Dr. Wollaston's scale that the requisite

proportions are 8·4 parts of acid to 100 of salt." Notwithstanding your remark, you surely cannot suppose that in directing these relative quantities, the College could be ignorant of the proportion of the ingredients of muriate of soda. Permit me then to remind you, that the numbers in the scale indicate the quantity of sulphuric acid necessary to saturate a given quantity of soda, but by no means the quantity of this acid necessary to effect the decomposition of the muriate with the greatest success and convenience. It must, I presume, for the moment have escaped your recollection that sulphuric acid is much disposed to form a supersulphate of soda, and consequently that if no more acid be employed than is barely sufficient to saturate the quantity of soda contained in the muriate, a considerable portion of the muriate will remain undecomposed.

After many repetitions of the process, I may confidently assert that the proportions assigned in the *Pharmacopœia* afford a larger product of muriatic acid in a shorter period at a smaller expense of fuel than those which you recommend as the requisite.

Acidum Nitrosum.—I suspect that we can turn to little account the remarks which you have made on the process for preparing this substance, as I am persuaded from ample experience that they are not correct either in regard to the quantity of the product, or the condition of the acid which is obtained. You have thought fit to condemn the proportions directed in the *Pharmacopœia* as unproductive and injudicious on the result of a solitary trial, which you state in the following words: "I put into a retort 24 parts of nitre and 16 of sulphuric acid, and carried on the distillation as long as nitric acid was produced. The product was of a straw colour, evidently containing but very little nitrous acid, and its specific gravity was 1513 instead of 1520, as stated in the *Pharmacopœia*. It weighed 11·5 parts, whereas 24 parts of nitre are capable of yielding 17 parts of acid, provided sufficient sulphuric acid is employed to afford water enough to condense the nitric acid." The results which I have had uniformly for many years are extremely different. The quantity of acid amounts to 15 parts; it possesses a full orange-red colour; and its specific gravity, never less than 1520, occasionally (when the nitre has been previously well dried, and the sulphuric acid boiled) has been so high as 1540.

I cannot refrain from expressing my surprise at the following paragraph: "I have already observed that the acid which I obtained has only a straw colour instead of a red one, as the College seemed to expect, and I believe that whenever this acid has this red colour, it is owing to the presence of common salt in the nitre, the chlorine of which partially decomposes the nitric acid."

Every chemist knows that nitric acid acquires a red colour by the action of muriatic acid (not of chlorine as you by inadvert-

186. Dr. Hope's Remarks on Mr. Phillips's Analysis [MARCH, once here state), which causes a partial decomposition; but it is altogether a mistake to suppose that the red colour of the acid got by decomposing nitre proceeds from this source. Hundreds of chemists, I presume, as well as myself, have, since the days of Glauber, been in the habit of employing purified nitre, and have obtained a red coloured acid. It must surely have escaped your memory that the red coloured acid is the *spiritus nitri fumans*, or Glauber's spirit of nitre, the only form of the concentrated acid familiar to chemists till about 40 years ago.

In the commencement of the distillation, the acid is pale, and continues so till towards the close; then, provided the temperature has been raised to the due pitch, it acquires the orange tinge by absorbing the ruddy fumes which now appear accompanied with oxygen gas proceeding from the partial decomposition of part of the acid.

As you state that you cannot see why the Edinburgh College should employ the particular proportions directed by them, I beg leave to mention the reasons; first, the prescribed quantity of sulphuric acid is required, and proves sufficient to detach the whole of the acid from the nitre; secondly, the acid thus procured is of great strength, and is so free from sulphuric acid, as to render the second distillation enjoined by the London College altogether unnecessary for ordinary purposes.

Acidum Nitricum.—The criticism of this article appears to me to be incorrect in every point. Having this moment called your attention to the mistake in regard to the colour and quantity of the acid, I need not recur to them. Though for many purposes the nitric and nitrous acids may be used indiscriminately, yet there are some where they cannot with propriety; and the object of the formula is, to direct the apothecary how he can procure a colourless acid; while at the same time he obtains a portion of the acid in its most nitrous condition. I presume it is from your never having seen the red acid got by our process that you have been led to remark that the red acid when diluted is quite pale; whereas, in truth, if it be diluted as directed by the Pharmacopœia, it is of a rich green colour. By free exposure indeed to the air, this colour gradually, but very slowly, disappears.

You have assigned as a reason for preferring the process of the London College, should a pale acid be wanted, that it affords it by one operation, while that of the Edinburgh requires two. But you have forgotten that the London Pharmacopœia directs a second distillation of a fresh quantity of nitre, and after all does not procure a colourless acid.

Aqua Potassæ.—You object to our process, because thrice as much lime is used as the theoretical quantity; and because the excess occasions waste by the quantity of the solution retained. Experimentally I have found by many trials, that this excess of lime deprives the potassa much more completely of its carbonic

and then a smaller quantity; and that there is no waste of the solution, provided the filtration be practised as the Pharmacopœia directs; for by the successive affusions of water, the whole of the solution is pushed through the lime.

Subcarbonas Ammonia.—You allege that two parts of carbonate of lime are ordered when one and a half ought to be sufficient, and that the excess requires the use of larger vessels and more firing. The object of the excess in this case is to secure the decomposition of the whole of the muriate, and save both time and fuel.

Aqua Ammonia.—This is a substance the preparation of which has been ordered in the different Pharmacopœias in a variety of ways. Though you particularly condemn our process, yet I am persuaded, from many comparative trials, that it merits the preference. Your first objection lies against the excess of lime; but unquestionably, in this instance, it is particularly useful, in accelerating the disengagement of the ammonia, and rendering a less elevation of temperature necessary. You also complain that the pungency of the odour on mixing and introducing the materials into the retort causes much annoyance to the operator. The odour is unquestionably pungent, but if the operation be adroitly managed, it causes no annoyance of the smallest consideration.

When the quantity of materials is not large, the mixture may be made, and introduced so speedily, as to avert every inconvenience; and if it be intended that the charge of the retort shall be considerable, it will require no great sagacity in the practical chemist to discover, that he can mix the articles in as small portions as he finds convenient, and introduce them in succession. While you give a preference to the formula of the London College, you admit, that estimating by the strength of the product merely, that of Edinburgh is more economical in the proportion of 16 to 10; but you add, “as, however, the Edinburgh order the large quantity of lime, which they employ with the muriate of ammonia, to be made red-hot, the retort generally breaks; and as this does not follow as a matter of course in the London process, I consider it to be really most economical.” Had indeed the Edinburgh College directed the lime and the muriate to be made red-hot, as you have here inadvertently stated, the retort would almost infallibly give way, and the balance struck by you might then be a fair one; the College, however, have given no such directions; the words are “*tandem supponatur ignis sensim augendus donec fundus olla ferrea rubescat et quamdiu gas et humor prodierint.*” As thus the heat is to be increased only till the bottom of the sand-pot grows red; and as the retort has always a quantity of sand interposed; and further, as much of the heat is carried off by the gas, the temperature of the mixture is, in every stage very far distant from the point of incandescence; indeed the whole ammonia is disengaged long before the temperature could be elevated to that point; and the Phar-

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macopœia directs the process to be stopped as soon as the whole
of it has come over.

The consequence is, that the retort is in no hazard of being broken. I have employed the same retort many times; and in fact I know no process in which a retort is less likely to suffer than the one now under consideration.

You have also said that having tried the processes of the London and Edinburgh Colleges, you certainly find that of the London more easily managed. In my trials, the reverse has been the case. In the London mode, in addition to the trouble of filtrating, much more attention is requisite in the management of the distillation. If the retort and receiver be closely luted, which is often done to prevent the escape and loss of ammonia, the heat must be regulated with the utmost care, else there is a great risk of bursting the vessels. If they be not closely joined, the loss of ammonia is considerable. The Edinburgh process is not liable to inconvenience from either of these sources, and the general advantages which I have found it to possess are, that it is the most economical both in regard to the quantity of the product, and the time and fuel required to obtain it; and that it is managed with less trouble, without being liable to the chance of having the retort broken, either by bursting, or by any other cause peculiar to the process.

Tartras Antimonii.—The name given to this substance naturally first excites your animadversion. Had you happened to look at the preface to the Pharmacopœia, you would have found the reasons assigned by the College for deviating occasionally from strict nomenclature, and abbreviating the name of some compound substances, for the sake of convenience in prescription, by restricting it to that of the active ingredient. The *Tartras Antimonii* is one example.

There is no preparation in the Pharmacopœia for which so many and so varied formulas have been proposed as the *tartras antimonii*; and upon this article I shall only remark, that with the aid of Dr. Duncan, jun. Professor of the Institutes of Medicine, all the processes, lately recommended by the Colleges of London and of Dublin, and by yourself, were carefully tried; and that we saw no reason for preferring any of them to the one in our former edition.

Carbonas Ferri Præcipitatus.—As I have not lately performed the process for obtaining this substance with a view of ascertaining the proper proportions, I shall not presume to assert that in this instance also you have been unfortunate in your criticism. At all events, the difference between the quantity of subcarbonate of soda, ordered by the Edinburgh College, and that which you commend, is so inconsiderable, that it may easily have arisen from the different condition of the subcarbonate employed in regard to water of crystallization.

Acetas Hydrargyri.—You profess to be ignorant why the

College orders a slight excess of acid. Allow me, therefore, to inform you, that when a solution of the nitrate of mercury without excess is poured into a solution of the acetate of potassa, a turbidity and deposition of subnitrate of mercury commonly take place; and the proper product is injured, unless the liquor be immediately filtered as the Dublin College directs. The slight excess of acid completely guards against this occurrence.

The quantity of water directed by the *Pharmacopœia* is not, as you allege, too large; it is required to keep the acetate of mercury in solution at the moment of its formation, and thus secures its perfect crystallization.

Murias Hydrargyri Corrosivus.—The fault which you have stated to the direction for this substance is the excess of the common salt; but you are certainly in a mistake in asserting that it is totally useless. Here I am persuaded it is particularly required that no portion of the sulphate may suffer the subliming temperature free from the contact of muriate of soda.

Submurias Hydrargyri Præcipitatus.—Since you admit the assigned proportions of acid and mercury to be correct, little comment on your remark is required. The College, however, is not inconsistent, as you assert, in ordering some excess of nitric acid for the acetate, and forbidding it for this preparation; because the excess is advantageous for the one, and prejudicial for the other.

Oxidum Hydrargyri Cinereum.—As you have already acknowledged with much candour, in p. 144 of No. II. of the *New Series of the Annals*, that you had committed an error in greatly overrating the quantity of earth in the solution of lime, and have in consequence withdrawn your objection to the formula for this substance, it is unnecessary for me to say any thing in its defence.

Oxidum Hydrargyri Rubrum per Acidum Nitricum.—If the diluted nitric acid have been formed from the strongest acid, you are perfectly correct in saying that it will dissolve an equal weight of mercury, and in that case more acid is ordered by the *Edinburgh College* than is necessary for preparing this substance. But if the best acid usually met with in apothecaries' shops be employed, in consequence of its inferior strength, the proportion assigned in the *Pharmacopœia* will be found most suitable.

Acetas Plumbi.—In the formula for this substance I acknowledge that you have detected an oversight on the part of the College. I trust, however, it is a very venial one, as it consists merely in employing the older instead of the new name for the oxide of lead used in this preparation.

I have now gone through the different articles of your criticism, and with the exception of the trivial oversight in nomenclature now admitted, I trust that every chemist who shall peruse these remarks will be satisfied that none of your objections are well founded. I am very far from thinking that our *Pharmacopœia*

194 *Dr. Iveson the Chemical Properties and Economical* [MARCH, is free from blemish, though I am convinced that the College took great pains to render it so. To them it must be satisfactory and gratifying that after so long and deliberate a consideration of its merits, you have not discovered in the department which has excited your attention any real imperfection. I earnestly beg that you will take the trouble of repeating those processes, of which you have expressed your disapprobation, and faithfully observe the directions of our Pharmacopœia, when I am fully persuaded you will be convinced that your strictures have been misapplied.

Though in repelling your attack, I have been under the necessity of pointing out numerous mistakes into which you have fallen, yet I entertain that opinion of your candour and justice, as to leave me no doubt of your giving a place to this letter in the next number of the *Annals of Philosophy*. An opportunity will thus be afforded to those who have read your analysis of seeing also the reply to it. I have the honour to be, Sir,

Your very obedient servant,

THOMAS CHARLES HOPE.

ARTICLE IV.

*An Experimental Inquiry in the Chemical Properties and Economical and Medicinal Virtues of the Humulus Lupulus, or Common Hop.** By Ansel W. Ives, MD. of New York.

THE hop is a hardy perennial plant, which grows spontaneously in the northern parts of Europe and America.† It belongs to the class Diœcia, and order Pentandria, of Linnæus. The plant which bears the male flowers is not cultivated, and is called the *wild hop*.‡ The common domestic hop, which is the female plant, is now to be the subject of investigation. Its general character is too well and too universally known to need description. The hop § has been regarded from time immemorial as an indispensable ingredient in malt liquors. It was introduced and cultivated for that purpose in England about the year 1549, and has since been used so extensively in that country and in many others, as to have become an important article of commerce. It has long been known also to possess some virtue

* From an American Scientific Journal.

† That the *Humulus* is a native of America has been confirmed by the observation of Michaux, Nuttall, Eaton, Torrey, and others.

‡ A very accurate drawing and minute dissection of the male and female hop-plant may be found in "Lamarch's Encyclopedia," part 22, plate 815.

§ Writers have generally used the term *hop-plant* to distinguish the whole vegetable, and the *hop* to designate that part of it used in brewing.

as a medicine, and a general description of its character and properties is recorded in most Pharmacopœias.

Not having seen any accurate analysis of this article, and considering it important that the physician should know in what part of the plant its medicinal virtue resides, I commenced some experiments with a view to ascertain this object. The facts which were developed in the progress of the investigation were to me novel and unexpected; and the results to which they obviously led altogether different from what I had anticipated. The *medicinal* character of the hop was, therefore, now regarded as a subject of minor importance; for however desirable might be the merit of introducing to general use a new and eligible *form* of medicine, that consideration would excite comparatively but little solicitude, while there existed a hope of effecting an improvement in domestic economy, which would be materially interesting to a great portion of the civilized world.

A quantity of hops was procured which had been kept for domestic purposes in a small bag for three years. When they were taken from the bag, there remained about two ounces of an impalpable yellow powder, which, by sifting, was rendered perfectly pure. This substance has probably been observed by most persons acquainted with the hop, and I suspect has generally been mistaken for *pollen*, but it is peculiar to the female plant, and is probably secreted by the nectaria. It seems to have been more correctly appreciated by those accustomed to the domestic use of hops, than by many others, professing a more scientific knowledge of their culture, properties, and use. I have not been able to find any notice of this powder in books, and know not that it has been designated by any appropriate term. In the following inquiry, therefore, it will be called *Lupulin*.

Exp. 1.—One dram of lupulin was boiled with two ounces of water, in a small retort, till a third part of the water had passed over into a receiver. The fluid that came over indicated slightly the peculiar aromatic flavour of the hop; it was perfectly transparent, very little discoloured, and exhibited no appearance of a volatile oil. The water remaining in the retort was aromatic and bitter. When filtered and evaporated, it yielded 10 grs. of a pale yellow extract, intensely bitter, and possessing in a high degree the peculiar aromatic taste of the hop.

Exp. 2.—Two ounces of the best merchantable hops were distilled in a retort with six ounces of water till half of the fluid had passed over into a receiver of water. The water in the receiver was slightly impregnated with the odour of the hop, but there were no appearances of volatile oil.

Exp. 3.—Two drams of lupulin were boiled in a retort with three ounces of alcohol. The alcohol came over strongly impregnated with the aroma of the lupulin; but there was no visible indication of an essential oil. The remaining alcohol had assumed a brilliant yellow colour, and a pleasant but intensely

196 *Dr. Ives on the Chemical Properties and Economical* [MARCH,
bitter taste ; when filtered and evaporated, it yielded one dram
of extract, of the consistence of soft wax.*

Exp. 4.—A saturated decoction of the lupulin was prepared with pure water. It was opaque, and of a pale yellow colour. By adding to a portion of it a solution of the sulphate of iron, the colour was changed to a deep purple, approaching to black ; a solution of animal gelatine threw down a copious ash-coloured precipitate, which left the supernatant liquor transparent and clear. This liquor was now decanted ; by adding to it a solution of iron, it was changed to a pale blue ; the acetate and subacetate of lead caused a copious curdy yellow precipitate ; the nitrate of silver, a greenish flocculent precipitate ; muriate of tin, when first added, produced no change, but after standing a short time, a brown precipitate ; a solution of sulphate of alumine caused no immediate change, but by boiling with the decoction, it separated a dense precipitate. Silicated potash, alcohol, and vegetable blue, induced no change.

Exp. 5.—Two drams of lupulin in four ounces of water were digested six hours in a sand-bath. The infusion yielded by evaporation six grains of aromatic and bitter extract. Two ounces of proof spirit were added to the same lupulin, and subjected to a moderate heat 12 hours ; when filtered and evaporated, there remained six grains of a resinous extract. The same lupulin was digested 30 minutes in boiling alcohol, from which was obtained by evaporation 62 grs. of extract. The extract obtained by the second process was soluble in pure alcohol, and when water was added to the solution, it became turbid and milky.

Exp. 6.—The lupulin used in the last experiment was boiled in strong caustic ammonia. When filtered and supersaturated with distilled vinegar, a copious precipitate ensued, which was insoluble in alcohol, and possessed the sensible properties of an impure wax. The last three experiments show pretty satisfactorily that the most important proximate principles of the lupulin are resin, wax, tannin, gallic acid, a bitter principle, and an extractive matter. The following experiments were instituted for the purpose of ascertaining more accurately their respective proportions as well as the aggregate amount of soluble matter in a given quantity of lupulin.

Exp. 7.—Two drams of lupulin were infused five hours in boiling water. To the filtrated infusion were added, at intervals, five grains of animal gelatin in solution, when it ceased to produce any precipitate, and the supernatant liquor became transparent and clear. The sediment, when dry, weighed 10 grains. An ounce of alcohol was added to the filtered solution, but it

* These experiments, with some variation, were frequently repeated, with the view to detect, if practicable, the volatile oil which is so frequently mentioned by authors as essential to the flavour of beer. The result was uniformly the same. The peculiar aroma of the hop was always obvious to the smell and taste, but I was never able to separate it in the form of an essential oil.

caused no change; by evaporation, it yielded 15 grs. of a very bitter extract. The same lupulin was digested again in boiling water—animal gelatin added to the filtered solution, induced no precipitate; by evaporation, an additional quantity of six grs. of the watery extract was obtained.

Exp. 8.—The extract obtained in the last experiment, was put into pure alcohol, and frequently agitated. After 24 hours it was filtered; 10 grs. had been redissolved by the alcohol, and an insoluble mass, weighing 11 grs. was left upon the filter.

Exp. 9.—The same lupulin which was used in the seventh experiment was now digested in alcohol. The infusion was highly bitter, and of a fine yellow colour; it gave by evaporation 24 grs. of resin. By digesting in a second portion of alcohol, 12 grs. more of resin were obtained, less bitter, but otherwise like the last.

Exp. 10.—The lupulin which was the subject of the last experiment, after having been thus boiled in water, and digested in alcohol, was put into a small retort, and boiled in two ounces of ether. While boiling, it was filtered into a vessel containing cold water, by which means 12 grs. of wax were obtained.*

Exp. 11.—Half an ounce of lupulin was boiled successively in water, alcohol, and ether. On weighing the insoluble residuum, it was found that five-eighths of the whole had been taken up by the solvents.

From the foregoing experiments, all of which were, with some variation, frequently repeated, I infer, that the lupulin contains a very subtle aroma, which is yielded to water and to alcohol, and which is rapidly dissipated by a high heat; that no essential oil can be detected by distillation in any portion of the hop; that the lupulin contains an extractive matter, which is soluble only in water; that it contains tannin, gallic acid, and a bitter principle, which are soluble in water and in alcohol; that it contains resin, which is dissolved by alcohol and by ether, and wax, which is soluble only in alkalies and in boiling ether; that it contains neither mucilage, gum, nor gum-resin; that the aromatic and bitter properties of the lupulin are more readily and completely imbibed by alcohol than by water, and much sooner by both when they are hot than when they are cold; that about five-eighths of the whole substance is soluble in water, alcohol, and ether, there being about three-eighths of it vegetable fibrous matter. These proximate principles exist in very nearly the

* The usual method of separating wax from vegetables, by boiling them in caustic ammonia, and then supersaturating the alkali with vinegar, or with diluted sulphuric acid, is tedious, and the results unsatisfactory. The following is a much more easy and beautiful process. After digesting the substance in boiling water and cold alcohol, let it be boiled in ether, and the solution strained, while boiling, into cold water. The wax, which is held in solution by boiling ether, is thrown down as soon as the ether is cooled by the water, and its specific gravity being greater than that of ether, and less than that of water, it forms a beautiful partition between them. If the ether be suffered to evaporate, the wax may be taken from the water entire.

198 *Dr. Ives on the Chemical Properties and Economical* [MARCH, following proportions :—In two drams (or 120 grains) of lupulin, there is :

	Grains.
Tannin.	5
Extractive matter.....	10
Bitter principle.....	11
Wax.	12
Resin.....	36
A woody fibrous substance, or lignin....	46

Exp. 12.—Two drams of the leaves,* from which all the lupulin had been separated, were digested 12 hours in six ounces of boiling water. The infusion was bitter, and exceedingly unpleasant to the taste; it possessed none of the aromatic flavour and peculiar bitter of the lupulin. When filtered and evaporated, it yielded five grains of nauseous extract. The same leaves were again digested in six ounces of proof spirit: after 12 hours, the infusion was filtrated, and, by evaporation, yielded five grs. of extract, similar to the last. The same leaves were digested 24 hours in alcohol; the infusions manifested none of the sensible properties of the hop; it gave by evaporation four grains of extract. The taste of none of the extractive matter obtained from the leaves was sufficiently characteristic of the hop to designate that it was obtained from that article.†

From this and other similar experiments leading to the same results, I think it is conclusively proved, that the virtue of the hop resides exclusively in the lupulin; that the leaves contain a nauseous extractive matter, which is imparted to water and to alcohol, and which, instead of adding to the bitter and aromatic flavour of the lupulin, partially neutralizes or destroys it.

The obvious inference from these results was, that the lupulin was the only part of the hop essential to economical purposes; an inference so little anticipated, that it became an important subject of inquiry, whether that part of the plant was duly estimated by practical brewers—whether it had been regarded by authors as preferable to the leaves, and if so, what impediment or what consideration prevented its being separated from the chaff.

On making inquiry of a number of brewers in this city, it was ascertained that there was about one in three who considered this powder useful, in common with other parts of the plant. It was known to all that hops were used principally for their anti-septic powers, or to preserve the beer from acetous fermentation; but neither practical brewers, nor scientific writers on brewing,

* It will be understood that by the *leaves* are meant the calices which form the flower, or that part of the hop commonly used in brewing.

† It is necessary to remark that great care was taken to procure the leaves for this experiment perfectly free from the lupulin, which is ordinarily attached to them in great abundance. This cannot be done by threshing them.

appear to have noticed this substance particularly. By some of the former, it is regarded as useless. When at one brewery I asked for some of the yellow powder that was found at the bottom of the hop bags; I was told that I could find but little there, as but a few days ago they had swept half a bushel of it from the store.

I was now resolved to ascertain, if possible, the proportion of lupulin in the merchantable hop, and also whether it could be completely and readily separated from the leaves. Accordingly six pounds of pressed hops were taken from the centre of a bag, containing some hundred pounds, and exposed to heat till perfectly dry. They were then put into a light bag, and by threshing, rubbing, and sifting, 14 ounces of the pure powder were separated in a short time, and with very little labour.

Though the quantity thus obtained was surprisingly great, there was obviously a considerable proportion remaining which could not easily be separated from the chaff. If, therefore, the hops were gathered when the lupulin existed in the greatest abundance, and, instead of being pressed and packed, were exposed to the sun till perfectly dry, there is little doubt but six pounds would yield a pound of the powder in question.*

The foregoing experiments were not completed till late in the spring, when the best season for brewing was passed, but with the advice, and by the direction of Robert Barnes, Esq. (an experienced and scientific brewer, zealous for the improvement of his art) two barrels of beer were made in which nine ounces of the lupulin were substituted for five pounds (the ordinary quantity) of hops. The result confirmed the most sanguine expectation. Though the quantity of lupulin was less than what (according to the foregoing statement) usually enters into the same quantity of wort; and though the weather during the month of June was unusually warm, and, therefore, unfavourable to its preservation, still the beer, which is now five weeks old, is very fine. It is pleasantly aromatic and bitter, and in a perfect state of preservation.

To ascertain the preservative property of the lupulin by a more direct experiment, equal quantities of the beer were put into separate vials and exposed, unstopped to the sun. To the beer in one vial was added a scruple of lupulin. The beer, to which none was added, became mouldy and sour in 10 days; the other was unchanged at the expiration of 15 days.

Having, as I conceive, demonstrated that the lupulin alone contains the bitter principle and the aromatic flavour of the hop, which are essential to the excellence and preservation of malt liquor, and having shown also the feasibility of separating it from the leaves to which it is attached; I shall proceed to

* Nothing conjectural would here have been introduced, but with a view to show, as accurately as possible, the proportion of lupulin, that the requisite quantity may be known in case it should be substituted for the leaves of hops in brewing.

200 *Dr. Ives on the Chemical Properties and Economical* [MARCH, enumerate some of the most obvious benefits which would result from these facts should they be found applicable to practical use.

1. It would diminish the expenses of transportation. In this the saving would be enormous. The hops which are now brought to this city are cultivated in the eastern states, and in the western parts of this state, and the expense of transportation is from one to two cents a pound. This is on account of their bulk rather than their weight. Were the lupulin separated from the leaves, it being but about the sixth part in weight, and not one-twentieth in bulk, it might be compressed into casks, and thus transported with convenience and at a small expense. In short, the difference would not be less than that of sending wheat to market before and after threshing: Might it not also, for the same reason, become a profitable article of export?

2. It would lessen the difficulty and expense of storage. Notwithstanding the present mode of pressing hops into bags (which is done not less to diminish their bulk than to preserve their virtue) their storage is, as it ever has been, an important item of expense, as well as a very great burden to the brewer.

3. One object in pressing the hop into bags is, to preserve it from the injury of the air, a long exposure to which, it is said, partially destroys its virtue. Whatever may be the cause, it is well known that the value of hops is diminished by age. This could not result to the lupulin any more than to our imported teas, were it packed in casks which would secure it perfectly from the air.

4. The brewer would evade an enormous loss which he now sustains in the wort absorbed by the hops. Dr. Shannon, who has perhaps devoted more time and talent to the subject of brewing than any other English author, has demonstrated, by a series of experiments, that one barrel of wort is absorbed by every 60 pounds of hops in the ordinary process of brewing.* The quantity of beer manufactured annually in London is upwards of 1,500,000 barrels,† and the least quantity of hops used in making it is two and a half pounds to the barrel, or 3,750,000 pounds; now as a barrel of wort contains not less than three bushels of malt, it follows that the quantity of malt thus annually lost by absorption is 187,500 bushels—the price of which may be fairly estimated at as many dollars.

5. It will lessen the temptation to the fraudulent practice which now prevails of adulterating beer with other vegetable bitters. Notwithstanding the prohibitions of parliament there is no article which is the subject of such varied and extensive fraud in England at the present day as that of beer. As a substitute for the hop,‡ the cocculus indicus, quassia and wormwood

* Vide, Dr. Shannon's Treatise on Brewing.

† Edinburgh Encyclopedia, vol. ii.

‡ Accum's Treatise on the Adulteration of Food. Also, Edinburgh Review, No. 65.

have all in turn been used ; but all of them are so far inferior, both in their flavour and in their antiseptic or preservative properties, that the use of all vegetables in the manufacturing of beer, excepting malt and hops, is by law forbidden. By the improvement which is now proposed, so great would be the diminution in the price of the hop, from its being made an article of easy and cheap transportation, that there would be little inducement for using any other article in its stead.

6. The lupulin is exceedingly bitter, but not unpleasant, whereas the nauseous extractive matter of the leaves which, by boiling, is imparted to the beer, is unpleasant to the taste, and, when highly concentrated, is frequently ungrateful to the stomach. It is believed that few persons ever relished the peculiar bitter of the strong beer, until, by drinking it habitually, their taste becomes vitiated, as is the case in the use of opium and tobacco. Soon after hops were introduced into use in brewing in England, the citizens of London petitioned parliament to forbid their use in the kingdom, as they were a nuisance, "*and spoiled the taste of their drink.*" The leaves then are not only useless, but prejudicial to the flavour of beer.

On the virtues of this substance as a *medicine*, I shall at present be very brief, as it will probably be made a subject for future consideration. It has already been observed that the hop has long been regarded as a medicine of some value. In France it has been used as a tonic, and prescribed in dyspepsia and scrofula. In this country it has been most valued for its narcotic powers, and used in cases when opium was inadmissible. The most common preparation is a saturated tincture of the leaves. To this there are two important objections : 1. To give enough of the tincture of the leaves to induce sleep, the quantity of alcohol is necessarily so great as sometimes to do injury to the patient. 2. When given in large doses, it frequently produces nausea, and sometimes vomiting. The first of these objections requires no proof; the second is confirmed by my own observation, and by the experiments of Dr. Bryorley, in his inaugural dissertation on the hop. This last effect is probably owing to the extractive matter in the leaves, for I have never seen it produced by the lupulin. I have prescribed the powder in substance, the infusion, decoction, alcoholic tincture, and the extract. As its aromatic and bitter properties are imparted to water, the infusion is an eligible preparation as a tonic and stomachic ; but if given with a desire to produce sleep, the tincture is the best preparation. As it has been demonstrated, both by positive and negative testimony, that the narcotic principle exists in the *resin* only, the tincture should always be made with alcohol and not with proof spirit. It is more difficult and expensive to prepare the extract than the tincture, and the latter, in most instances, is the most eligible preparation.

Its virtues are aromatic, tonic, and narcotic ; and it is, I

202 *On Mixtures of Oxymuriate of Potass for Ordnance.* [MARCH, believe, the only article in which these properties are combined. Our country abounds with vegetable bitters and tonics, many of which are more powerful than the hop, but there is, perhaps, none which can so properly be denominated a stomachic. That family of symptomatic diseases which are the consequence of exhausted excitability, or more directly of an enfeebled and deranged state of the stomach and bowels, are certainly much relieved by this medicine. It frequently induces sleep, and quiets great nervous irritation, without causing costiveness, or impairing, like opium, the tone of the stomach, and thereby increasing the primary disease. As an anodyne, it will be found inefficient compared with opium. The saturated alcoholic tincture, in doses of from 40 to 80 drops, will induce sleep with as much certainty as opium in cases of long watching from nervous irritability; but the same cannot be said of its efficacy in relieving pain. This substance then is not commended as a medicine which ought to supersede the use of others of acknowledged virtue, but as a useful auxiliary, which undoubtedly possesses properties in some respect peculiar to itself, and as the part of the hop altogether preferable to any other, or to the whole as it is ordinarily used in tincture.

ARTICLE V.

On various Mixtures of Oxymuriate of Potass for discharging Ordnance.

(To the Editor of the *Annals of Philosophy.*)

SIR,

Feb. 20, 1821.

OBSERVING, in the last number of the *Annals of Philosophy*, some experiments on various mixtures of oxymuriate of potass for discharging ordnance according to Mr. Forsyth's plan, I take this opportunity to suggest a mixture of Howard's fulminating mercury, sulphur, and charcoal, separately reduced to powder, and afterwards intimately mixed. Several experiments on various mixtures of these three ingredients were made in Paris a short time since; but as they were conducted with great secrecy, I was unable to ascertain the use to which they were destined. I have seen some granulated after the manner of gunpowder, and some made perfectly spherical, in which latter form it appeared most energetic.

If this composition is not generally known, you will, perhaps, think it worthy a place in the *Annals*. I remain, yours, &c.

T. N. R. M.

P. S. The proportions of the ingredients best adapted to the above purpose might easily be ascertained by a few experiments.

ARTICLE VI.

Astronomical and Meteorological Observations.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude $51^{\circ} 37' 44.3''$ North. Longitude West in time $1^{\circ} 20.93''$.

Astronomical Observations, Feb. 6, 1821.

Occultation of δ by the moon. . . $\left\{ \begin{array}{l} \text{Immersion } 6^h \ 13' \ 28.7'' \\ \text{Emersion } 7 \ 07 \ 06.7 \end{array} \right\}$ Mean Time at Bushey.

These observations were made under very favourable circumstances, and the light of the star at the instant of immersion was as bright as when it was several minutes distant from the moon's limb.

Rain, between noon the 1st of January and noon the 1st of February, 2.115 inches. Evaporation, during the same period, 0.680 inch. Mean heat of January, 37° . Thermometer, lowest, Jan. 4th, = 21° . Highest, Jan. 18th, = $49\frac{1}{4}^{\circ}$.

ARTICLE VII.

On the Going of a Clock with a Wooden Pendulum.

By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Bushey Heath, Stanmore, Feb. 7, 1821.

IN the *Annals of Philosophy* for February, 1820, was published the going of a clock with a wooden pendulum, and also a description of its construction; but as it may be more satisfactory to have the account of the rate for a longer period than 12 months, I have the pleasure to send a table of the clock's going, during a second year, and the result corroborates the favourable impression I then entertained of its accuracy.

I remain, dear Sir, your obliged servant,

MARK BEAUFOY.

Table containing the Rate of a Clock with a Wooden Pendulum.

Date.	Clock fast or slow.	Difference.	Daily rate.	Date.	Clock fast or slow.	Difference.	Daily rate.
1830.				1820.			
Jan. 31	+ 0' 37-72"		- 0 45"	Sept. 21	+ 3' 27-09"	3 39"	+ 1 70"
Feb. 9	+ 0 33-64	4 08"	- 0 10	27	+ 3 30-21	3 12	+ 0 52
17	+ 0 32-84	0 80	+ 0 79	30	+ 3 30-52	0 31	+ 0 10
21	+ 0 35-99	3 15	+ 0 13	Oct. 3	+ 3 30-37	0 15	- 0 07
27	+ 0 36-75	0 76	+ 0 31	6	+ 3 32-54	2 17	+ 0 54
28	+ 0 37-37	0 92	+ 0 33	7	+ 3 31-52	1 02	- 1 02
29	+ 0 38-69	1 32	+ 0 61	11	+ 3 33-89	2 37	+ 0 59
March 4	+ 0 41-17	2 48	+ 0 79	12	+ 3 34-00	0 11	+ 0 11
8	+ 0 45-10	3 93	+ 0 04	16	+ 3 35-05	1 05	+ 0 26
13	+ 0 45-32	0 22	+ 0 73	18	+ 3 35-36	0 31	+ 0 16
18	+ 0 48-23	2 91	+ 1 85	25	+ 3 36-67	1 31	+ 0 19
22	+ 0 51-94	3 71	+ 2 19	28	+ 3 36-01	0 66	- 0 13
25	+ 0 54-13	2 19	+ 1 77	Nov. 2	+ 3 32-95	3 06	- 0 61
28	+ 0 59-13	5 30	+ 1 12	4	+ 3 31-28	1 67	- 0 84
30	+ 1 00-58	1 15	+ 0 57	7	+ 3 28-45	2 83	- 0 94
April 4	+ 1 06-18	5 60	+ 1 12	13	+ 3 21-69	6 76	- 1 35
7	+ 1 12-99	6 81	+ 2 27	15	+ 3 19-36	2 33	- 0 78
15	+ 1 22-39	9 40	+ 1 55	18	+ 3 16-60	2 76	- 0 92
19	+ 1 22-83	0 44	+ 0 11	26	+ 3 06-58	10 02	- 1 25
21	+ 1 24-70	1 97	+ 0 93	Dec. 1	+ 2 59-63	6 95	- 1 39
23	+ 1 28-05	3 35	+ 1 67	7	+ 2 54-38	5 25	- 0 84
25	+ 1 31-38	3 33	+ 1 66	14	+ 2 57-28	2 90	+ 0 41
28	+ 1 39-69	8 31	+ 2 77	21	+ 3 03-27	5 99	+ 0 85
May 1	+ 1 48 04	8 35	+ 2 78	26	+ 3 08-69	5 42	+ 1 08
6	+ 2 03-12	15 08	+ 3 02	29	+ 3 12-36	3 67	+ 1 22
9	+ 2 06-55	7 53	+ 2 51	31	+ 3 15-59	3 23	+ 1 61
13	+ 2 20-00	9 35	+ 1 56	1821.			
17	+ 2 22-37	2 37	+ 1 18	Jan. 13	+ 3 36-19	20 60	+ 1 59
21	+ 2 27-11	4 74	+ 1 18	17	+ 3 37-72	1 53	+ 0 38
25	+ 2 28-12	1 01	+ 1 01	23	+ 3 39-73	2 01	+ 0 40
27	+ 2 28-73	0 61	+ 0 61	29	+ 3 43-17	8 44	+ 1 21
29	+ 2 33-78	5 05	+ 1 26	Feb. 1	+ 3 50-40	2 23	+ 0 74
30	+ 2 37-85	4 07	+ 1 36	5	+ 3 54-59	4 19	+ 1 05
June 5	+ 2 42-64	4 79	+ 1 19	7	+ 3 37-96	3 37	+ 1 68
5	+ 2 45-19	2 55	+ 1 26				
17	+ 2 48-23	3 04	+ 0 25				

ARTICLE VIII.

*Table of the diurnal Variation of the Magnetic Needle, made at Zwanenburg, in Holland. Communicated by Col. Beaufoy.**

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Morn.	1775. 21° 38'	21° 36'	21° 38'	22° 3'	22° 3'	22° 0'	22° 1'	21° 57'	21° 39'	21° 38'	21° 29'	21° 33'
	1776. 21 37	21 48	21 45	21 51	21 49	22 19	21 54	22 6	21 28	21 17	21 9	21 40
	1777. 22 1	21 45	21 48	21 54	22 17	23 2	23 2	22 59	22 30	22 40	22 18	21 54
	1778. 22 42	22 11	22 14	22 47	22 39	22 35	22 34	22 42	23 4	22 53	22 32	22 40
	1780. 22 33	22 31	22 33	22 37	22 32	22 33	22 33	22 24	22 39	22 42	22 42	22 41
Noon.	1775. 21 39	21 37	21 47	22 12	22 9	22 5	22 6	22 6	21 43	21 43	21 30	21 37
	1776. 21 41	21 54	21 50	21 54	21 53	22 23	21 57	22 11	21 38	21 24	21 18	21 44
	1777. 22 3	21 47	21 50	21 53	22 20	23 2	23 3	23 0	22 32	22 41	22 19	21 58
	1778. 22 46	23 13	23 13	22 49	22 39	22 35	22 35	22 43	22 5	22 53	22 33	22 37
	1780. 22 33	22 32	22 35	22 37	22 32	22 34	22 34	22 24	22 40	22 43	22 43	22 40
Even.	1775. 21 37	21 35	21 40	22 6	22 8	22 4	22 3	22 1	21 39	21 41	21 27	21 35
	1776. 21 38	21 47	21 44	21 52	21 50	22 41	21 55	22 10	21 32	21 19	21 10	21 42
	1777. 22 00	21 45	21 47	21 55	22 19	23 0	23 1	22 59	22 30	22 38	22 17	21 56
	1778. 22 45	23 12	23 11	22 48	22 38	22 34	22 33	22 41	22 51	22 3	22 32	22 36
	1780. 22 31	22 30	22 34	22 37	22 32	22 34	22 33	22 23	22 39	22 42	22 42	22 39
									22 33	22 34	22 36	22 35

* For this Table I am indebted to Astronomer Charles Remeker, Director of the Nautical Academy at Hamburg.—M. R.

ARTICLE IX.

Remarks regarding the Experiments upon Flame, performed with the Apparatus for discharging Ordnance without the Use of a Light or Match-Lock. By Mr. John Deuchar, Lecturer on Chemistry in Edinburgh.

(To the Editor of the *Annals of Philosophy*.)

SIR,

27, Lothian-street, Edinburgh, Feb. 10, 1821.

AT a meeting of the Wernerian Natural History Society, which was held to day, I read the continuation of my account of the experiments performed with the apparatus for discharging ordnance, a description of which was given in the last number of the *Annals*. In this paper I have entered upon the cause of the results which present themselves. I have endeavoured to show by experiments that it is not in the slightest degree electrical, but that it may, with more propriety, be ascribed to free caloric in a so far insulated or condensed state. I have not yet tried the whole of the experiments I had chalked out to myself; for the proof of this, so far, however, as I have gone, they verify the conclusion. I have also noticed more fully the nature of the flame while in rapid motion, and the alterations of its effect upon substances by retarding that movement; when its force is not retarded, it passes through many inflammables without affecting them in the least; but when its velocity is so far stopped as to bring it for a longer time in contact with the substance to be acted upon, then it begins to display, in a greater or less degree, the usual effects of caloric.

As you will find the minute details of these experiments in the third volume of the Transactions of the Wernerian Natural History Society, to be published in a few days, I should not have troubled you with any observations at present, had it not been that from what was mentioned at p. 93 of the present volume of the *Annals*, regarding the experiments with wire gauze, it might appear that the gunpowder could not be fired through the wire gauze used in Sir H. Davy's safety lamp, without the wire being injured. I have since found that the wire gauze I bought of the manufacturer, and which he assured me was the same as that used in the safety lamp, was by far too fine, and that it was on this account the flame sometimes forced away a part of it. The following experiments, which I extract from the paper above alluded to, will remove any misconception that may have arisen on this part of the investigation. The wire gauze I used was made of brass; an inch of the finest kind contained 70 meshes in the length, being 4,900 in the square; and the coarsest kind contained 36 in the length, being 1,296 meshes in the square

inch. Now upon examining two of the lamps recommended by Sir H. Davy, the one with copper gauze, and the other with iron gauze, I found the former had only 676 meshes in the square inch, and the latter 784 meshes in the same space.

Experiment 13.—A tube which could be separated into six pieces of nearly the same length, was screwed to the apparatus (Plate III, p. 89), making the distance from the top, A, to the bottom fully 23 inches. A piece of the coarser wire gauze, already described, was put upon the hole at the joining, *a*, when the fulminating powder was exploded at A, the flame passed through the gauze, and appeared at the bottom of the tube. The same kind of wire gauze was next placed at *a* and *b* at the same time, and then at *a*, *b*, and *c*, and the flame passed through all the pieces. This effect was also obtained when similar pieces of wire gauze were put at all the five joinings of the tube at once. In this last result, the first piece of wire gauze was $4\frac{1}{4}$ inches from the top, A; the second, $8\frac{1}{4}$; the third, 12; the fourth, 16; and the fifth, 20; and the flame appeared at the bottom, after a passage of nearly $23\frac{1}{4}$ inches, through five pieces of the wire gauze.

Experiment 14.—As I could not get the flame to pass through the whole of the tube, when I increased the joinings beyond $23\frac{1}{4}$ inches, it was impossible to try an additional number of pieces of wire gauze, by adding them in the same way. I, therefore, increased the number by putting more than one at the same joining. I found, upon repeated trials in this way, using the tube 15 inches long (as represented on the plate, p. 89, fig. 1), that the flame could pass through three, six, nine, and twelve pieces at once: there being placed one, two, three, and four pieces at each of the joinings, *a*, *b*, and *c*.

Experiment 15.—Although, by the two last experiments, it was proved that the flame could pass through the coarser wire gauze when increased even to 12 pieces at once, yet it did not follow that it was not thereby altered somewhat in its nature. A probable change was, that it might become inert with regard to inflammables, as takes place in the different safety lamps, and particularly that of Sir H. Davy. Several experiments were tried to ascertain if this suggestion were correct; first, the wire gauze was put at *a*; then at *a* and *b*; and lastly, at *a*, *b*, and *c*; placing at the same time, during each trial, a quantity of gunpowder in a piece of flannel at the bottom of the tube; and in all of these I found the gunpowder to be inflamed, and the wire gauze not to be in the least injured.

Experiment 16.—I next tried the result of firing the fulminating powder, with the finest wire gauze placed first at *a*, then at *a* and *b*, and then at *a*, *b*, and *c*, and found that the flame still appeared at the bottom, B; showing that the gauze, although much finer than that used in Sir H. Davy's safety lamp, was not impervious to this flame. In some of the experiments I found a

208 *Machine to measure a Ship's Way by the Log Line.* [MARCH, hole to have been made in the centre of the wire gauze, and sometimes the parallel wires were forced wider. This was very often the case when a piece of wire gauze was put at all the joinings, *a*, *b*, and *c*, and then it was the gauze at *a* which was torn or otherwise injured.

Experiment 17.—In order to ascertain if the flame still remained unaltered, notwithstanding its having passed through the finest wire gauze, a quantity of gunpowder in flannel was affixed to the bottom of the apparatus; and it was inflamed through one, two, and even three pieces of the gauze. Here the same occasional appearance, noticed in the last experiment, occurred with regard to the upper piece of wire gauze.

I am, yours respectfully,
JOHN DEUCHAR.

ARTICLE X.

On a Machine to measure a Ship's Way by the Log Line.
By Mr. J. Newman

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

I OBSERVE in the last number of the *Annals of Philosophy* a paper referring to the account I have given of a machine to measure a ship's way by the log line, in which a prior claim to mine is set up for the invention. This I conclude to arise from the circumstance of my not having mentioned the date of the instruments I made; and from it your correspondent supposes that the date of the publication of the paper is that of the invention.

It is now at least 40 years ago since I invented and made the first instrument in question. Capt. Hubbard, then of the East India service, had the first of them, and between the years 1785 and 1798 they were publicly sold by Messrs. P. and J. Dollond, and Mr. George Adams, with their names on them; but my account book of that time is destroyed. My present account book begins in 1794, and I find that Messrs. Dollonds had one of the instruments on Sept. 4, 1797; and another on the 27th of the same month. These dates can no doubt be easily verified by reference to their books. I made several, perhaps 16 or 18, between the dates I have mentioned, but my attention being called to other things, I did not pursue the subject with any degree of earnestness. I remember that Capt. Clayton had one of the first that I made in the year 1785, and he recommended me to take it to Sir Charles Middleton, at the Navy Office,

Somerset House, who spoke well of it, and advised me to take it to the Admiralty ; but I did not pursue the matter further.

As the only question is merely as to the date of the invention, and not to any imaginary superiority in principle, I need not direct attention to the supposed advantage of either one or the other instruments.

I am, dear Sir,

Very respectfully yours, &c.

J. NEWMAN.

ARTICLE XI.

On the comparative Advantages of illuminating by Gas produced from Oil and from Coal. By M. Ricardo, Esq.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

THE utility of employing gas for the purpose of illumination is no longer a subject of doubt, but from what substance it is most advantageously obtained, whether by decomposing oil or coal, is not yet decided by men of science. I have, therefore, been induced to enter into an examination of the comparative advantages of the two in order to draw the attention of those who are engaged in the formation of gas light establishments to the subject, and to enable them to arrive at a tolerably correct opinion, which it would be most advisable to adopt.

It is my intention to consider

1. The qualities of the two gases for producing light.
2. The comparative facilities with which an establishment for the production of either may be carried on.
3. The capital required for such establishments.
4. The comparative cost of the two gases.
5. Which is most desirable in a national point of view.

The gas produced from oil is much purer, and contains a much greater illuminating power than that from coal. The quantity of light produced from a given portion of oil gas is stated by an eminent chemist to be equal to three times the quantity produced from coal gas : from the result of my own experiments, it is equal to four times ; for I have found that an Argand burner giving a light equal to six candles, six to the pound, consumed one cubical foot in the hour. Mr. Accum, in his work on Gas Lights, p. 276, states that an Argand burner of coal gas giving a light equal to three candles, eight to the pound, consumes two cubical feet per hour. Then as one foot of oil gas is equal to six candles, and two feet of coal gas are required to equal three candles, it follows, if the candles were even of the same size, that one volume of oil gas is equal to four of coal in illuminating

power. If we take the mean of these statements, it will be as one to three and a half; that is, 20 cube feet of oil gas will give as much light as 70 of coal gas.

Oil gas requires no purification; it contains no sulphuretted hydrogen which is one of the admixtures of coal gas, and of this all the purification to which it is submitted cannot wholly deprive it; the coal gas, therefore, acts upon all metallic substances, and in the course of time must seriously injure the pipes through which it passes, and its accidental escape in shops and houses must prove highly detrimental to all ornamental gildings, paintings, or any thing of which metals form a part. This cannot happen where oil gas is used; for it contains no sulphuretted hydrogen, and it is well known to have no action on metals whatever. It may be said, that the mode adopted for purifying coal gas effectually deprives it of this noxious gas; but experience has proved that this is not the fact, as in many places the smaller copper pipes show evident marks of being strongly acted upon, the bore being gradually filled up with sulphuret of copper. As this process takes place slowly, the diminution of light is not immediately perceptible, but it will become very evident after a time; and it may be expected that after the lapse of a still longer period, the same evil may arise in the larger pipes, as iron is also liable to the same corrosive influence from its contact with this gas. The admixture of sulphuretted hydrogen with coal gas must prevent its general introduction into houses, as the sulphurous acid gas, which is given out during its combustion, would prove very annoying in a confined room, besides which, from the consumption of so much larger a portion of coal than oil gas to produce the same light, a greater quantity of moisture is generated, and much more heat is given out.

From the above statement, it is very evident that the smaller bulk and greater purity of oil gas will allow of its employment in dwelling houses without its producing the least inconvenience. If the pipes are well fitted together and properly proved before the gas is admitted into them, no annoyance whatsoever need be apprehended; and if a cock should be accidentally left open, and the gas allowed to escape, it may be immediately remedied without leaving so unpleasant a smell as that arising from the similar escape of coal gas.

When any new improvement is introduced, we are too apt to place any possible inconvenience that may result from it in an exaggerated point of view, and wholly to overlook those we are labouring under, and which it is intended to supersede. Thus the only possible inconvenience that can result from the use of oil gas is that which I have before mentioned, an accidental smell from carelessness, instantly detected, and as instantly remedied—an inconvenience to which we are equally liable in our present mode of lighting, in addition to which there is the trouble of trimming the lamps, the chance of spilling the oil, not a very unfrequent

occurrence, and many other disasters both in the use of oil and candles, from all of which gas is exempted; and when the prejudice arising from the use of coal gas is once removed, and the greater advantages of oil gas generally known, there is no doubt its great convenience, superior brilliancy, and cheapness, will cause it to supersede all other modes of lighting, and before long, its universal introduction into dwelling houses may be confidently relied upon.

The second point for consideration is the comparative facility with which a coal or oil gas establishment may be carried on. The process for producing gas from oil is very simple. The apparatus is easily managed, and consists of an iron retort heated over a fire: a very few days' experience will teach a common working man how to regulate the heat. This retort is connected by a tube with the oil cistern, from which a small drop of oil passes into it. The quantity is regulated by a graduated cock; there it is decomposed and converted into gas. This passes through another tube at the other extremity of the retort into a condensing vessel, where, by a simple contrivance, the oil that is only volatilized returns back into the oil cistern; the gas is then conveyed into a wash vessel, where it passes through water to deposit any oil, or other condensible vapour, that may have come over with it, and from thence it is conducted into the gasometer for use.

Coal gas is produced by putting a certain quantity of coal in an iron retort placed over a fire. The coal is decomposed; the gas passes over into a large vessel, where it deposits its tar and ammoniacal liquor; it is then conveyed through a mixture of lime and water to deprive it of the sulphuretted hydrogen which is mixed with it, a most troublesome and offensive operation: after this, it is passed through water, where it is more effectually washed, and from thence it is transmitted into the gasometer. These are the processes required for producing the oil and coal gas; but we shall better understand the trouble attendant on the latter by a comparative view of two establishments for a thousand lights each, one for oil, and the other for coal gas, each light consuming annually, upon an average, 2000 cube feet of oil gas, and 7000 cube feet of coal gas. The whole annual consumption of the one would be 2,000,000, of the other 7,000,000 cube feet. The quantity of light required in winter is of course much greater than in summer, and either establishment must be made upon a scale to meet the demand that may be necessary on the shortest days. The fortnight before and after Christmas, or about that time, may be taken as the month of the greatest consumption; and we may assume that during this month, nearly one-fourth of the whole quantity would be required. I may mistake in this estimate, but it can be of no consequence, as it will equally apply to both establishments. The average quantity of oil gas required would be, during that time, somewhat above 16,000 cube feet

per night, and 56,000 of coal gas. To produce that quantity of the former, eight or 10 retorts would be sufficient, each retort six feet long, and six inches diameter. One of this dimension would, if necessary, produce 3000 feet per day, but it is found more advisable not to work them to their full extent, and always to have some in reserve. One gasometer containing 12,000 cube feet; or two of 6000 each, would be required. The oil cistern, the condenser, and wash vessel, are so constructed as to occupy but very little space, the condenser being over the cistern, and the wash vessel under it, the gasometer occupying by far the largest portion of room. From the repeated trials in various oil gas establishments, it has been ascertained that 10 gallons of oil produce 1000 cube feet of gas, and require one bushel of coals for decomposition: 2,000,000 feet of oil gas, therefore, would require 20,000 gallons of oil, weighing between 78 and 79 tons, and 74 tons of coal; so that about 153 tons of materials would be annually wanted in this establishment, all of which would be consumed, or converted into gas.

In the coal gas works, 40 retorts at least would be required.* Each retort is six feet long, and one foot in diameter, and would be charged with two bushels of coals, which quantity would require eight hours for decomposition. At the end of this time, the coke would be removed, and fresh coals put in. The above-mentioned quantity of coals would give out 560 cube feet of gas, estimating 10,000 cube feet for every chaldron. The utmost quantity of gas which one retort could produce would be 1680 cube feet, working night and day without intermission: 34 is the number actually necessary; but in an establishment like this, the wear and tear would always be requiring some to be replaced, I have, therefore, taken the number at 40. The vessel to receive the condensed tar, oil, and ammoniacal liquor, must be of considerable size, as also those for burning the gas, and for washing it. One gasometer of 40,000 cubic feet, or two of 20,000 each, would be scarcely sufficient. To produce 7,000,000 cube feet of coal gas would require 700 chaldron of coals, and 175 for carbonisation, weighing altogether 1,181 tons, besides 50 tons of lime for purifying.

In a coal gas establishment there will be a conveyance required for 1230 tons of materials, the greater part of which are returnable in bulky articles, such as coke, tar, ammoniacal liquor, for each of which a market must be found, as the greater part of our estimated profit is derivable from them; and of the two latter articles, the product is already so great as very far to exceed any possible demand for them: so that a further conveyance is required for more than three-fourths of the above estimated materials, and large premises and reservoirs to contain them till disposed of. Added to these, there is produced a very

* See Peckston on Gas Lights.

large quantity of hydrosulphuret of lime, which is extremely offensive and quite useless. In the oil gas establishment, the conveyance of 153 tons of materials is all that is required, the whole of which is consumed, and the entire profit derived from one product only, and that the sole object of manufacture.

From the above investigation, it is clear that on this point also the oil gas must be infinitely preferable as deriving its profit from the consumption of materials of small bulk, and the product being only one article readily disposed of. It is, therefore, I think, evident, that such an establishment must be carried on with much less trouble than where the materials are extremely bulky, and the profits derivable from various articles, some of which are disposed of with difficulty, and are together almost as bulky as those from which they were originally produced.

From the foregoing statements, it must clearly appear that the capital required for the establishment of an oil gas work must be considerably less than would be necessary for one of coal gas. The fewer retorts, the smaller size of the gasometers and conveyance pipes, the much greater simplicity, and consequently cheapness of the apparatus, together with the very little labour and superintendence required in works even upon a very large scale, prove clearly, and without any further comment, that the capital requisite in the one will, as already mentioned, bear no proportion to that necessary in the other. This is an object of some consequence to those embarking in such an undertaking, as the risk of loss in case of failure is comparatively small, and consequently less difficulty will be found in forming an oil than a coal gas establishment.

The next subject for consideration is which is the more economical method? and it may appear surprising to many that light from oil gas can be afforded to the consumer as cheap as that from coal, and at the same time yield as great or greater profit to the Company supplying it. This is, however, the case, though we have not such satisfactory data to go upon as to make it clearly demonstrable, from being unable to ascertain what the profits of the Coal Gas Companies really are, and which can only be assumed from the known dividends which are made upon their shares. The difficulty of calculating profits must always be increased when they are derivable from an average of various articles, for some of which there is a fluctuating demand, and which, from their increased production, are likely to diminish in value. From some known data, it is supposed that the cost of coal gas to the Companies, reckoning the sale of coke, tar, &c. would be about 10s. per 1000 cube feet; the selling price is estimated at the rate of 15s.*: it may be more, but is certainly not

* Since writing the above, I have been informed, on good authority, that in some of the provincial cities and towns, the selling price of coal gas is estimated much higher than 15s. per 1000 cube feet. This is what may be expected, for should the supply of coal gas become much greater, in consequence of an increased demand for it, without

less than that. This would give them a profit of 50 per cent. upon their capital. The highest return quoted, which is of the Bristol Gas Works, is but 10 per cent.; and the chartered Company in London, which possesses advantages superior to any, only divides eight per cent. It is difficult to account for this; but on the supposition that the making of the gas must cost them much more, or that there must be some mismanagement, or considerable waste. We will, however, assume that it only costs 10s. per 1000 cube feet.

The profit and loss account of an oil gas establishment may be calculated very easily. Ten gallons of oil will produce 1000 cubic feet: this may be considered the average quantity, and taken from the result of various trials where oil gas is used. Oil suitable for the purpose may be now procured for 20*l.* per ton, but as the price of oil at present is very low, and may not be considered a fair average, we will take it at 25*l.* per ton. or about 2*s.* per gallon. The cost then for producing 1000 cube feet of oil gas will be as follows:

	£	s.	d.
10 gallons of oil at 2 <i>s.</i> per gallon	1	0	0
1 bushel of coals	0	1	6
Labour, wear and tear, and contingencies. ..	0	5	6
	<hr/>		
	1	7	0

At the present price of oil, the cost would be much less. Should oil advance beyond the sum at which I have averaged it, it will be of course greater; it may also be thought that I have underrated the expenses of wear and tear, labour, and contingencies; but the estimate is founded upon the data afforded by works upon a small scale, and their expences generally exceed those of a larger description. Further, to meet those objections, I will take the cost of 1000 cube feet at 30*s.*: this is equivalent to 3,500 cube feet of coal gas, which at 10*s.* per 1000 will cost 35*s.* giving a balance in favour of oil gas of 5*s.* upon those proportionate quantities.

In enumerating the advantages of oil gas over coal, it may not be irrelevant to consider it in a national point of view, as a nursery for our seamen: supposing in every other respect the balance of advantages to be equal, this would give the oil gas a decided preference. I am aware that this advantage may be purchased too dearly, and that when coal gas was first introduced, its great superiority over other modes of lighting made it desirable that the benefit resulting from the fisheries should in a degree be relinquished, but now that other circumstances concur in making

an adequate interest demand for coals, and the price of the gas will fall, while the price of the latter articles will fall, and the loss sustained upon those articles must be made up in the advanced price of the gas. This inconvenience would be felt, not much by the small establishments, but the new ones.

ing the use of oil preferable, there can be no doubt of the propriety of considering this as an additional motive for preferring oil gas.

It has been suggested by some intelligent gentlemen, whether, if the use of oil gas should become very general, the supply of oil would be adequate to the demand; and whether, from the increased demand, the price would not be considerably enhanced. When coal gas was introduced, the demand for oil was diminished; and the capital employed in that trade was diverted into other channels for the supply of the coal gas. It is very clear that should oil gas become generally adopted, part of this capital will again revert to its former employment as far as it supersedes coal gas; but where it only displaces lamps, no change will ensue; and where it is substituted for candles, the use of tallow will be exchanged for that of oil.

After perusing the foregoing observations upon the comparative advantages of oil and coal gas, and which I have endeavoured fairly to state, I think it will be generally inferred that the former is greatly preferable to the latter; that on every point in which I have examined them, it has the advantage; that it is much purer and better adapted for every purpose of lighting; that it is prepared with much greater facility; that it requires much less capital to establish works; that it may be produced more economically; and that it is also more advantageous in a national point of view.

I feel some reluctance in offering this paper to you for insertion in the *Annals of Philosophy*, as the subject of it cannot be considered as purely scientific; but as the object of science is to add to and improve our comforts and enjoyments, it may not be wholly foreign to your object to point out what scientific improvements are most likely to effect this. From the same motive I might also be deterred from subscribing my name, but the bare supposition that you, or your readers, should, for a moment, conceive that the writer of this was influenced by interested motives in seeking publicity to it, would induce me to overcome every feeling of reluctance on that head. I trust my name will be a sufficient guarantee that I have no other motive for publishing this than that which every one interested in scientific improvements, and their adaptation to the uses and comforts of society, must feel.

M. RICARDO.

ARTICLE XII.

On a peculiar Substance obtained during the Distillation of Nitric Acid. By M. Julin, of Abo.

I HAVE observed during the distillation of nitric acid from a mixture of crude nitre with calcined sulphate of iron, that when a peculiar kind of the calcined vitriol (known in Sweden under the name of calcined aquafortis vitriol, No. 3*) was used, the first conducting tube† was lined with a yellow substance, which proved to be sulphur; the second tube became internally covered with fine white feathery crystals, nearly resembling the icy film formed upon windows on a cold winter morning. I collected this substance by washing the tube with water; the quantity obtained by each distillation was exceedingly small, amounting to only a few grains. This substance is white, consists of small fibres, which adhere to each other, and it feels soft to the touch; it sinks slowly in water, and is insoluble in it, whether cold or boiling, but the vapour of boiling water carries up a small portion of it.

It is tasteless, but has a very peculiar smell, which I have some difficulty in comparing with any other substance, but it somewhat resembles that of spermaceti. Muriatic acid does not act upon it; nor does nitric acid of specific gravity 1.45, but the acid, after having boiled with it, showed some slight traces of sulphuric acid on the addition of nitrate of barytes. When boiled in concentrated sulphuric acid, it sublimed through it unchanged.

A strong solution of caustic potash dissolved a very inconsiderable portion of it, and gave with acetic acid a trifling precipitate; which was too small to be examined, excepting as to colour, and from this and a slight smell of sulphuretted hydrogen gas, I judged it to be sulphur.

Oil of turpentine dissolves it easily, when heated. On cooling, a great part crystallizes in small needles; it also dissolves easily in boiling alcohol of 0.816. On cooling, the greater part is deposited in crystalline fibres, but a small portion remains dissolved; for the solution, on being poured into water, renders it turbid. The alcoholic solution, when distilled with a gentle heat, left the greatest part of this substance in the retort, but the distilled alcohol still retained enough to render water turbid by precipitation.

* This vitriol crystallizes in the water of the mine of Fahlun, and collected from the rock after the water has been pumped out, it is impure, and contains generally a small portion of pyrites.

† The distillation is carried on in an iron retort, with a receiver connected by glass tubes in the manner of a Woulfe's apparatus.

Held in the flame of a lamp, it burned with a greenish-blue flame, giving a slight smell of oxymuriatic gas; and when heated in a glass tube, it melted, boiled, and sublimed in needles, at a heat between 350° or 400° , and it sublimed slowly in long needles without melting at a heat of about 250° .

Potassium burned with a vivid flame in its vapour in an open tube; a great quantity of carbon was deposited, and water poured upon it gave, after saturation with nitric acid, a copious precipitate with nitrate of silver.

The small quantity which I have as yet obtained of this substance, and want of leisure, have prevented me from making more experiments upon it; but I purpose doing it as soon as the one and the other are more at my disposal: till then, I suspend forming any opinion of its composition. In some respects, it seems to resemble the perchloride of carbon which Mr. Faraday has lately succeeded in producing, and has so ingeniously examined.

It is rather strange that sulphur should sublime unaltered in the vapour of nitric acid, as I have mentioned to occur in the case of this distillation of nitric acid, but it is really the fact, and may be accounted for by want of water for the sulphuric acid to combine with.*

Experiments on the above described Substance. Ry R. Phillips, FRSE. &c. and M. Faraday, Chemical Assistant at the Royal Institution.

In order to purify this substance, a small quantity of it was passed in a state of vapour over hot lime. By this operation, a little sulphur, muriatic acid, and other impurities, were separated. The pure substance obtained was dried over sulphuric acid in the receiver of the air-pump, and it had then a white crystalline appearance; by subjecting it to heat, it was volatilized, and then condensed, in beautiful acicular crystals.

In the substance thus purified, there remained no traces either of chlorine or sulphur, as described by M. Julien, and it exhibited the following properties: It burned with a strong bright flame; at a heat below redness, it sublimed without undergoing any change; when passed through a red-hot green glass tube, there was a slight appearance of decomposition, a small quantity of charcoal being deposited, and the substance became brown, but

* On his departure for the Continent, M. Julien left the above account, and also some of the substance which he has described, in the hands of the Editor, with permission to make use of both as he wished. From the circumstance mentioned by M. Julien of its appearing to resemble the perchloride of carbon, the substance in question was shown to M. Faraday, and with his assistance the additional experiments now described were made.

238 *On a peculiar Substance found in Nitric Acid.* [Marens,
is crystallized and appeared to have suffered but little change; it
was soluble in alcohol and ether, as described by M. Julien.

A portion was repeatedly sublimed in a small retort filled with
chlorine, which was in several parts made red-hot, but even at
this high temperature, it suffered no change by the action of the
chlorine, and when left to cool, it reappeared in its original state
of small white crystals; a minute portion was raised in vapour
over mercury, mixed with oxygen gas, and detonated by the
electric spark; no charcoal was separated, and we found that an
excess of oxygen had been employed. The volume of residual
gas was precisely equal to that of the oxygen employed. This
gas, excepting a small portion of pure oxygen, was absorbed by
lime water, in which it caused a white precipitate, and conse-
quently it appeared to be carbonic acid gas. This experiment
was several times repeated with similar results.

A small quantity of this substance was passed in vapour over
red-hot peroxide of copper; a considerable portion was volatili-
zed without being decomposed; the gas obtained from that
part which suffered decomposition possessed the characters of
carbonic acid.

The very minute quantity of the substance which remained
prevented any further attempt to examine its nature; and the
knowledge we have of the mode and circumstances of its forma-
tion has not been sufficient to enable us to form it.

Although it would be premature to give a strong opinion as
to the nature of this substance, yet the striking peculiarities it
presents with heat, chlorine, and oxygen, have made us anxious
to form some idea of its composition. We are acquainted with no
compound of hydrogen that would present similar phenomena
when heated in chlorine gas, nor do any of the compounds of
carbon exhibit the same results with oxygen gas.

The substance in question may possibly be a compound of
carbon with oxygen and hydrogen, in the proportions required
to form water, and the circumstance of there being change of
volume on detonation with oxygen gas, might, on this supposi-
tion, be accounted for; but it is difficult to suppose that such a
compound would pass through a red-hot tube, or suffer exposure
to a red heat on chlorine gas without decomposition.

It may be allowed us to remark, in the uncertainty which we
feel respecting the nature of this substance, that the appearances
which it presents would agree with the idea of its being a simple
body: in this case, it may be either a new form of carbon, or a
peculiar substance analogous to carbon. We venture, however,
these ideas without putting any confidence in them. We enter-
tain hopes of being able to procure more of the substance, and
we shall then examine its properties with minute attention, and
determine the characters of the products which it gives by deto-
nation and other modes of treatment.

ARTICLE XIII.

Researches on the Composition of the Prussiates, or ferruginous Hydrocyanates. By J. Berzelius. (From the *Annales de Chimie*, vol. xv. p. 144, New Series.)*

THE discovery of cyanogen and of the composition of hydrocyanic acid, which we owe to the genius of M. Gay-Lussac, is certainly one of the most interesting that has been made in our time, particularly since this acid is found on the confines between those combinations which have an organic origin, and those whose origin is purely inorganic. The comparison of cyanogen with those bodies, which become acids by their combination with hydrogen, has simplified the theory of hydrocyanic acid, and of the phenomena produced by its decomposition; but nevertheless, the researches subsequent to those of M. Gay-Lussac, whose object was to throw more light on the subject, have by no means been made with sufficient care, fully to explain the nature of the salts called ferruginous hydrocyanates, the most important of the combinations of hydrocyanic acid.

We owe a multitude of important observations on the nature of these salts to Mr. Porrett. He found that the hydrocyanates can combine with sulphur, forming a particular acid, whose existence, and some of its properties, were known before; but in what the difference between this and hydrocyanic acid consists was far from being suspected. Mr. Porrett was led, moreover, to conclude from his experiment, that the iron which enters into the composition of the ferruginous hydrocyanates, does not exist in them in the state of oxide, but in its metallic form combined with the carbon, hydrogen, and azote; and that it is, consequently, one of the elements of the acid in which the iron plays an analogous part to that of the sulphur in the preceding compounds; and, therefore, an idea of the hydrocyanates being salts with two bases, of which oxide of iron is always one, is incorrect. Several memoirs have been published on this subject by Mr. Porrett, in Thomson's *Annals*, the principal results of which I shall detail.

He at first found ferruginous hydrocyanate of potash composed of

Ferruginous hydrocyanic acid (ferruginous elyzic acid)	47.66
Potash	30.34
Water	13.00

The same salt, analyzed by M. Ittner, gave

* Translated from the *Memoirs of the Academy of Sciences of Stockholm*, for the year 1819, p. 242.

Hydrocyanic acid	11
Hydrocyanate of protoxide of iron	38
Potash.	39
Water..	12

Thomson, who adopted Porrett's idea of the value of the feruginous hydrocyanic acid, examined the composition of hydrocyanate of potash, and obtained from it the following results :

Acid { Iron	13·0*	}	45·90*
Gas	30·9		
Potash			41·64
Water			13·00

Dr. Thomson states that during the decomposition of this salt by another acid, a portion of the hydrocyanate of iron is volatilized with such rapidity that when he poured nitric acid on the pulverized hydrocyanate, the acid in the flask from which he poured it was coloured blue by the volatilized prussiate of iron. Dr. Thomson endeavoured to analyze the acid, by burning the salt with base of potash in the oxide of copper in a tube of the same metal. Five grains of the crystallized salt gave 5·205 cubic inches of carbonic acid gas, and 2·42 cubic inches of azote, with 2·2 grs. of water, 0·65 gr. of which were derived from the water of crystallization of the salt. This experiment gave $2\frac{1}{4}$ volumes of carbonic acid gas for one volume of azote, exclusive of the carbonic acid retained by the alkali, of which no account was taken, nor, as far as appears, any deduction made, for the atmospheric acid contained in the apparatus at the beginning of the experiment.

Dr. Thomson concludes from it, that the acid of these salts contains, besides the iron, the same elements, and in the same proportion as the hydrocyanic acid, because he supposes that one fourth of the volume of carbonic acid gas, that he found in excess, might be an error of observation, and that the quantity of water, which is four times greater than the quantity admitted by this supposition, was derived from the corks with which the opening of the copper tube was closed, so that its hygrometrical water might have mixed with the products of the analysis. However when Thomson made his calculations from the results, and distilled the carbon and hydrogen, which did not coincide with the theory, the weight of the iron did not accord with the chemical proportions. "This," he says, "is the first compound which I have met with that does not seem reconcilable to the atomic theory. I invite chemists to the further investigation of it. There are no facts so likely to lead to the improvement of the science of chemistry as those which contradict our received

* So in the French.

opinions,"* to which it appears to me he should have added, "with the exception of those cases in which the discordance is the result of an ill-made, incorrect experiment," whenever the science derives a truly negative advantage.

Mr. Porrett also soon endeavoured to prove Thomson's analysis inaccurate. He, on this occasion, analyzed the salt of potash by a solution of tartaric acid in alcohol, and determined the quantity of potash by the supertartrate of potash produced. The following is the result of his analysis :

Ferruginous hydrocyanic acid.	50.93
Potash	35.48
Water	13

He had found that the ferruginous prussic acid might be insulated by this operation, and that it gave cubic crystals by spontaneous evaporation. In an analysis of this salt by oxide of copper, he obtained four volumes of carbonic acid gas for one of azote; whence he concluded that the acid is composed of one atom of azote, four atoms of carbon, one atom of hydrogen, and one atom of iron. He attributed the deficiency of carbonic acid in Thomson's results to his having employed too small a proportion of oxide of copper.

Lastly, Mr. Porrett published a further memoir on this subject, in which he again corrected his former results as follows :

Ferruginous hydrocyanic acid	{ Gas .. 32.72 Iron .. 12.60 }	45.82
Potash		41.68
Water		13.00

Repeating the experiment of burning the salt with oxide of copper, he constantly obtained four volumes of carbonic acid gas for one of azote; whence he concluded that the acid is composed of four atoms of carbon, one atom of azote, one atom of hydrogen, and half an atom of iron; but this half atom of iron not being consistent with the atomic views, he conceived his experiments to be sufficiently exact and positive, to decide, contrary to conclusions derived from less complicated and easier experiments made directly on iron and its oxides, that the weight of the atom of iron is not half what it has heretofore been admitted to be, or one-fourth of that laid down in my tables; whence, he concludes that the protoxide of this metal is composed of two atoms of base and one of oxygen, and the deutoxide of four of base and three of oxygen.

M. Vauquelin also made many researches on the prussiates; and his treatise on the subject is full of interesting facts; but he did not employ himself in determining the proportions of its elements, which is the principal object of the present memoir. He found that prussian blue, contrary to what Gay-Lussac had

222 *Mr. Perkins on the Compressibility of Water.* [MARCEL, endeavoured to render probable, is a prussiate, and not a cyanuret. Vauquelin thought, moreover, that he had discovered that those bases capable of decomposing water at the common temperature of the atmosphere give hydrocyanates, while the others give only cyanurets.

The last work on this subject that has come to my knowledge is a note by M. Robiquet on the composition of prussian blue. In it he confirms the results which M. Proust had derived from his experiments on the prussiates, made long since, and among others this, that the white prussiate of iron contains potash. M. Robiquet showed that this prussiate without potash may be obtained in the form of small crystalline grains, of a yellow colour, by exposing prussian blue for a long time to the action of sulphuretted hydrogen gas. He considers prussian blue as a combination of cyanuret of iron with a prussiate of deutoxide of iron and water; and he attributes its blue colour to water. He asserts that ferruginous prussiate of potash burned by means of oxide of copper always afforded him the gases in the same proportion to one another that Gay-Lussac found them in cyanogen; and he maintains that in this experiment the base retains no carbonic acid, as Mr. Porrett had noticed.

Results so contradictory, and conjectures so little justified by experiment, are not very well calculated to give us an exact idea of the composition of these salts; and although the path has been marked out by the labours of Gay-Lussac, we must confess, in spite of what has been done with him, that we are just at the same point at which he left the question.

M. Proust long ago proved the ferruginous prussiates to contain iron, and that they must be regarded as salts with double bases, of which the protoxide of iron is always one, exactly as alumina is always one of the bases in the different kinds of alum; and he showed that prussian blue must be a hydrocyanate in which the deutoxide of iron represents the other bases with the protoxide. Mr. Porrett's idea that the iron is an element of the acid has always appeared to me analogous to that of considering the potash in cream of tartar as an element of the acid in pel de singlette, or tartar emetic.

(To be continued.)

ARTICLE XIV.

On the Compressibility of Water. By Mr. Perkins.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Feb. 23, 1821.

IN the *Annals of Philosophy* for February, Dr. Roget has discovered a very material error in my computation on the first experiment made with the piezometer on the compressibility of

water. The real error was in the data being incorrect in the paper read before the Royal Society. The dimensions of the piezometer should have been as follows :

The diameter of the cylinder $3\frac{1}{8}$ inches, length $23\frac{1}{4}$ inches, the plunger $\frac{1}{8}$ diameter. The mistake originated by taking the dimensions of a bathometer which was used at the time the experiment was made.

With much esteem, yours truly,

JACOB PERKINS.

ARTICLE XV.

ANALYSES OF BOOKS.

Pharmacologia, or the History of Medicinal Substances, with a View to establish the Art of prescribing and of composing extemporaneous Formulæ upon fixed and scientific Principles; illustrated by Formulæ, in which the Intention of each Element is designated by Key Letters. By John Ayerton Paris, MD. FLS. MRI. &c. &c.

If this publication were entirely medical, it would of course be improper to notice it in a work professedly scientific; but as it involves considerations, and to a very considerable extent, which belong to the science of chemistry and the operations of pharmacy, I conceive that some account of it may not be misplaced in the *Annals*.

Dr. Paris has prefixed to this work an "Historical Introduction," giving an account of the more prominent revolutions that have occurred in the reputation of medicinal substances; and in the execution of this task, he has deviated from the beaten path, and has given an air of originality to a subject which might be considered as "somewhat musty." This history will be found, to use the author's own words, "an analytical inquiry into the more remarkable causes which have, in different ages and countries, operated in producing the revolutions that characterise the history of medicinal substances:" it will be impossible to read this introduction without fully assenting to an opinion expressed by the late Dr. George Fordyce, that whenever a substance was good for nothing else, it was tried in medicine. "The revolutions and vicissitudes," says the author, "which remedies have undergone in medical, as well as popular, opinion, from the ignorance of some ages, the learning of others, the superstition of the weak, and the designs of the crafty, afford an ample subject for philosophical reflection; some of these revolutions I shall proceed to investigate, classing them under the prominent causes which have produced them; viz. Superstition—Credulity—

Scepticism—False theory—Devotion to authority and established routine—The assigning to art that which was the effect of unassisted nature—The assigning to peculiar substances properties deduced from experiments made on inferior animals—Ambiguity of nomenclature—The progress of botanical science—The application and misapplication of chemical philosophy—The influence of climate and seasons on diseases, as well as on the properties and operations of their remedies—The ignorant preparation or fraudulent adulteration of medicines—The unseasonable collection of those remedies which are of vegetable origin; and the obscurity which has attended the operation of compound medicines." Now to this truly formidable list of mischief-producing causes, I think the author might have added, the careless revision of national Pharmacopœias; unless this may be classed under the head of the ignorant preparation of medicine.

Under the head of "Ambiguity of nomenclature," Dr. Paris has collected some curious facts, and from such various sources, as to evince that his reading has been extensive, and that he has neglected no means of attaining knowledge in illustration of his subject.

After mentioning some circumstances respecting the sweet and common potatoe, he observes, "A similar instance is presented to us in the culinary vegetable, well known under the name of Jerusalem artichoke, which derived its appellation in consequence of its flavour having been considered like that of the common artichoke; it is hardly necessary to observe that it has no botanic relation whatever to such a plant, it being an heliotrope (*heliotropium tuberosum*); the epithet *Jerusalem* is a curious corruption of the Italian term *gira-sole*; that is, turn-sun in English, or heliotrope in Greek."

Dr. Paris occasionally enlivens the subjects on which he is treating (and it must be confessed that they sometimes stand in need of it) with anecdotes which happily illustrate his positions: he remarks, that "it ought not to be forgotten that cultivation and artificial habits may have blunted the susceptibility of our organs, and in some instances changed and depraved their functions: certain qualities, for instance, are so strongly connected with each other by the chain of association that by presenting only one to the mind, the other links follow in succession. This might be illustrated," continues Dr. Paris, "by the recital of numerous fallacies to which our most simple perceptions are exposed from the powers of association; but I will relate an anecdote which, to my mind, elucidates the nature and extent of such fallacies more strikingly than any example which could be adduced. Shortly after Sir Humphry Davy had succeeded in decomposing the fixed alkalies, a portion of *potassium* was placed in the hand of one of our most distinguished chemists, with a query as to its nature? The philosopher, observing its

aspect and splendour, did not hesitate in pronouncing it to be metallic; and, uniting at once the idea of weight with that of metal, the evidence of his senses was even insufficient to discover ideas so inseparably associated in his mind, and, balancing the specimen on his fingers, he exclaimed, "It is certainly metallic, and *very ponderous!*" Now this anecdote is not related in disparagement to the philosopher in question. Who could have been prepared to meet with a substance, so novel and anomalous, as to overturn every preconceived notion? A metal so light as to swim upon water, and so inflammable as to catch fire by the contact of ice!

In the same strain it is further and happily observed by Dr. Paris, when treating of mercury, that "mythologists inform us that he was the winged messenger of the gods, and the patron of thieves. What name, therefore, could be more appropriate for the metal in question than that of this deity? for it is not only distinguished from all other metals by its mobility, but its universal agency has rendered it the resource of those worst of thieves—quacks, and nostrum-mongers." Dr. Paris, in addition to his own experience, and to the information gained by his reading on the subject, has obtained much useful information from persons in distant countries respecting the influence of soil, culture, climate, and season: for particulars, I must refer the reader to the work itself.

The only remaining part of the historical introduction which I shall notice, is that which treats of the application and misapplication of chemical science. In the beginning of this section, some very amusing and curious matter will be found. After noticing the works of Roger Bacon, Basil Valentine, Paracelsus, Van Helmont, &c. he comes down to our own times, and honours, first, Mr. Brande, and then me, with some notice for the criticisms which we have occasionally ventured to make upon the London Pharmacopœia; and I believe I may regard myself, without any undue pretensions, as a very prominent member of a new order denominated, by Dr. Paris, "*Ultra Chemists*;" and I am accused of exhibiting, in my Experimental Examination of the Pharmacopœia Londinensis, a "caustic style of criticism," rather than "any fatal or material inaccuracy" in the work reviewed. Great allowance is to be made for Dr. Paris, in passing his judgment upon my examination, for he is a Fellow of the College; but still not having been so at the time when the Pharmacopœia in question was edited, I think he would have shown more discretion in suffering it to sleep quietly in the dust to which the College have consigned it. I am not so good a judge of the appearances which indicate what is "fatal" as the Doctor; but, I think, if he should find that a person was utterly incapable of performing any of the functions which indicate vitality, he would conclude him to be dead. If then I discover in a Pharmacopœia *atarctarised antimony* which is not tartar *emetic*, and consequently incapable of answering the end for which it was prepared, I have

grounds for concluding that I have pointed out "a fatal and material inaccuracy." Indeed the College seem to have concurred with me on this point, for the process of which I complained has been abandoned. Whether that which is substituted for it is any better, I need not on the present occasion inquire.

To proceed, however, with that part of the work which it is more important to notice, it is to be observed, that after the historical introduction, we have the term 'Pharmacologia' defined, as comprehending "the scientific methods of administering medicinal bodies, and explaining the object and theory of their operation." This is divided into two parts; "the first comprehending the principles of the art of combination, and the second the medicinal history and chemical habitudes of the bodies which are the subjects of such combination."

Excepting a short and incomplete paper by the late Dr. George Fordyce, I believe no attempt has before been made to investigate the medicinal properties resulting from the mutual action of bodies independently of their chemical action. In the execution of this part of his subject, the author has developed some original views which appear to be capable of application to the chemical analysis of vegetable substances. Thus in mentioning senna, he observes, that its leaves "appear to contain an active principle in combination with a bitter, which latter ingredient, although destitute of purgative properties, considerably increases those of the former; for if this be removed, as happens when senna is transplanted into the south of France, the purgative principle is weakened, but may be again restored by the artificial addition of some bitter extractive." This and similar instances induce Dr. Paris to inquire whether it does not appear that "certain elements exist in the composition of vegetable remedies as furnished by nature, which, although individually inert, confer additional strength and impulse upon the principle of activity with which they are associated?"

Incurring the risk of being again deemed an "*Ultra*," I shall venture to observe that this reasoning appears to be derived from the well-known chemical fact, that it is impossible to discover, *a priori*, what will be the result of mixture; and I am apprehensive that the few remarks which I intend to offer upon the formulæ introduced by Dr. Paris, will be considered as irrelevant.

It will be seen by referring to the work that "key letters," so denominated, are placed opposite to each ingredient of a collection of formulæ to denote the mode in which it acts. Now although these formulæ appear in general to be extremely well composed, there are, I think, some instances in which the idea of the mutual assistance of similar medicines is carried a little too far. On this subject I confess I speak chemically and theoretically; but I would ask, what is there so different in the action of oak bark, galls, and catechu as astringents, that a mixture of them is preferable to any one, or at any rate any two of them;

but these three substances are employed together in one formula. It is indeed true that the oak bark is used in the state of an infusion, the galls in that of powder, and the catechu in the form of tincture; but as all of these bodies may be used in any of these forms, I cannot conceive the utility of using them all together. But this is, perhaps, the natural result of my being an "*Ultra.*"

Dr. Paris appears to have paid considerable attention to a subject of great importance: I allude to the methods of detecting the presence of arsenic; and he has pointed out, not only the methods by which it may be discovered, but has mentioned many circumstances which give rise to ambiguous appearances of its existence. In addition to the copper and silver tests, upon which he appears to place the greatest reliance, I would beg to suggest the additional evidence which may be easily and strikingly obtained by the use of an aqueous solution of sulphuretted hydrogen gas, with an aqueous solution of the suspected substance. It is, perhaps, scarcely necessary to observe, that the effect which results from their mutual action is the production of a yellow-coloured solution without any precipitate. From recent experiments, I am satisfied that *arsenious acid* possesses no alliaceous smell, and that it is peculiar to the arsenic volatilized in its metallic state. I mention this circumstance, because different opinions appear to be entertained on the subject; and if the arsenious acid be not heated under such circumstances as decompose it, a ready but rough method of detecting the presence of arsenic may be rendered useless.

As one of the not least useful parts of Dr. Paris's work, I may notice his exposure of quackery, and the statement which he has given of the composition of more than 100 of the most celebrated, and consequently the most mischievous, quack medicines.

In concluding this notice I may observe that with respect to those parts of this performance of which I may be supposed to be able to form an opinion, that opinion is highly favourable; and although the work is evidently intended to afford pharmaceutical and pharmacological information to the junior members of the medical profession, there are many of a maturer age who may receive much benefit from its perusal; and it will form a useful addition to the medical library.—*Ed.*

ARTICLE XVI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 1.—The Bakerian lecture on the best Kind of Steel, and Form for a Compass Needle, by Capt. Kater, was read.

Feb. 8.—A paper was read, on the Rosell Bones found in the Limestone Rock at Plymouth, by Mr. Whidby.

At the same meeting, a paper, by Dr. Henry, of Manchester, was partly read, on the Aeriform Compounds of Charcoal and Hydrogen, with some additional Experiments on the Gases from Oil and Coal.

Feb. 15.—The paper, by Dr. Henry, on the Aeriform Compounds of Charcoal and Hydrogen, was resumed.

At the same meeting, a paper was read, by the Rev. Dr. Robertson, entitled, "Observations of the Eclipse of the Sun on Sept. 7, 1820."

At the same meeting also a notice was read, respecting a lunar Volcano, by Capt. H. Kater.

Capt. K. first observed this volcano on Sunday, Feb. 4, the moon being then two days old; its appearance was that of a small nebula, of variable brightness, subtending an angle of 3" or 4". Its distance from the edge of the moon was 1.10th her diameter; and on the 6th, the angle it formed with a line joining the cusps was about 50° .

Feb. 22.—Dr. Henry's paper, on the Aeriform Compounds of Charcoal and Hydrogen, was concluded.

The object first proposed by the author was to examine the accuracy of those views of the compounds of charcoal and hydrogen which had arisen out of his former experiments, and those of Mr. Dalton, especially whether there be a compound answering in its characters to light carburetted hydrogen gas, the existence of which had been called in question in a late Bakerian lecture. This, after attentively, and at various times, examining the gas from stagnant water, he pronounces to be a distinct chemical compound, having uniformly the same composition and chemical properties, and the same specific gravity (0.556). It is constituted of 100 parts by weight of charcoal united with 33.40 of hydrogen; while olefiant gas consists of 10.0 charcoal + 16.70 hydrogen. Hence if the latter be considered as a compound of one atom of charcoal and one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal and two atoms of hydrogen; and as 100 cubic inches of carburetted hydrogen contain hydrogen equivalent to 200 cubic inches of hydrogen gas, he suggests the verification of the specific gravity of hydrogen gas by that of carburetted hydrogen, and finds that in this way it comes out 0.0698, making the relative weights of the atoms of hydrogen and oxygen very nearly as 1 to 8. The atom of charcoal also he estimates from the composition of carburetted hydrogen, and of carbonic acid, at 6.

His next experiments relate to the best means of analyzing mixtures of olefiant gas with hydrogen, carburetted hydrogen, or carbonic oxide; and of olefiant gas with all those three gases. Chlorine, he shows, may be employed with perfect accuracy, provided certain precautions are observed, which are described

and length in the paper. The chief of these is the complete exclusion of light, for in that case olefant gas alone is condensed, but even the faint light of a cloudy day was found sufficient to cause the speedy action of chlorine on the other gases. The paper contains also directions for analyzing mixtures of hydrogen, carburetted hydrogen, and carbonic oxide, but these, from their nature, are incapable of abridgment.

By the analytical processes thus established, he proceeds to examine the composition of oil gas and coal gas. The results are given in tables, but the general issue of the experiments is that oil gas (as he had formerly shown with respect to coal gas) is very far from being uniform in composition, but differs greatly in specific gravity and combustibility, when prepared at different times even from the same kind of oil, owing to variations of temperature and other circumstances. Essentially the gases from oil and from coal are composed of the same ingredients, though in different proportions, viz. simple hydrogen, light carburetted hydrogen, and carbonic oxide gases, with the addition of variable proportions of an elastic fluid, which agrees with olefant gas in being condensible by chlorine, but consumes more oxygen and gives more carbonic acid, by combustion, and has a higher specific gravity than olefant gas, and even than atmospheric air. Whether this ingredient be strictly a gas, permanent at all temperatures, or a mixture of olefant gas with some new gas, constituted of hydrogen and charcoal in different proportions from what are found in the known compounds of those elements, or merely the vapour of a volatile oil, he leaves to be decided by a future train of experiments.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Address read at the First Meeting of the Cambridge Philosophical Society, stating the Design and Objects of its Institution; written at the Request of the Council, by Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, &c. &c.

At the opening of the first meeting of the Cambridge Philosophical Society, the Members of the Council avail themselves of the earliest opportunity that has been offered to them, of expressing to the Society their congratulations upon its Institution. Convinced, as they all of them are, of the advantages likely to result from the establishment of such a Society, they do not hesitate to declare their opinion, that an event of more importance, as affecting the best interests of science, has rarely occurred in the annals of the University.

A century has now elapsed since the celebrated Woodward prefixed the following axiom to his "Essay upon the Natural History of the Earth," which took the lead in subjects of geological inquiry. "From a long train of experience," said he,

"the world is at length convinced that *observations* are the only sure grounds, whereon to build a lasting and substantial philosophy. All parties are so far agreed upon this matter, that it seems to be now the common sense of mankind.*" For this reason, when he composed his work, as he himself states, "He gave himself up to be guided wholly by matter of fact, intending to steer that course, which is agreed, of all hands, to be the best and surest; and not to offer any thing but what hath due warrant from observations.† Unfortunately, for the fame of this distinguished naturalist, and for the University to which he bequeathed his valuable collection, the want of a Society affording the means of philosophical communication, caused his immense treasure of facts to remain hoarded in a place by no means worthy of the collection, or convenient for its arrangement. Hence the hardly credible truths which are now beginning to come to light respecting the Woodwardian collection; hence the extraordinary circumstance, first made known by the late Professor, the Rev. J. Hailstone, that the corundum stone (a substance of such singular utility in the arts, and whose supposed discovery, as distinguished from other minerals, was attributed to Dr. Black, of Edinburgh), was not only known to Woodward, but specimens of it existed unnoticed in his cabinet many years before Dr. Anderson, of Madras, sent to Europe the examples upon which Dr. Black founded his observations. The same may be said with regard to other bodies, and especially that remarkable substance called the native meteoric iron of Pallas, also in the Woodwardian collection.‡ To obviate even the possibility of such occurrences in future, to lay open channels of communication for facts connected with the advancement of philosophy, and also to bring together men who are engaged in common pursuits of science, is the main object of the Cambridge Philosophical Society. The zeal and promptness which have been manifested in its establishment, and a view of the names which have been already added to the list of its members, excite a reasonable hope that, by means of it, a fund of valuable information may be gradually accumulated. Some idea may be formed of the usefulness of such an Institution, simply by referring to the various periodical Journals, edited, either by individuals, or by societies, in different districts of this kingdom; in which the philosophical

* Nat. Hist. of the Earth, p. 1. Lond. 1723.

† Ibid. "The observations I speak of," observes the same Author, p. 3, "were all made in England, the far greatest part whereof I travelled over on purpose to make them; professedly searching all places as I passed along, and taking a careful and exact view of Things on all hands as they presented; in order to inform myself of the present condition of the earth, and all Bodies contained in it, as far as either Grottos, or other Natural Caverns, or Mines, Quarries, Colepits, and the like, let me into it, and displayed to sight the interior parts of it."

‡ To prove this remarkable fact, Professor Hailstone purchased a specimen of the native meteoric iron of Pallas, and placed it in the Woodwardian collection by the side of Woodward's specimen, that their identity might be the more easily recognized.

contributions of the members of this University, being frittered and squandered away in detached and distant parts, appear to be almost without existence; but if the same scientific productions had been concentrated, their testimony of the industry and abilities of their authors would not only be creditable to the University, but would also tend more effectually to the advancement of Science. It is one of the objects of the Society, that a volume for giving publicity to such writings, should occasionally be sent forth, not at any fixed or stated periods, but so often as due and approved materials can be selected for this purpose, and to this end it is proposed that Philosophical Communications should be encouraged from every quarter likely to afford them, by rendering to their authors every possible assistance which may be necessary for their publication. Letters have been already transmitted from the Secretaries to persons who are likely to promote the intentions of the Society; and it is requested that all its members will themselves further the designs of the Institution, by inquiring for communications relating to the several branches of natural history, and natural philosophy, especially by means of their foreign correspondence, and the observations they may be able to collect from scientific men engaged in foreign travel. Whatsoever may tend to illustrate the history of the animal, the vegetable, or the mineral kingdom; of organized or of unorganized existences; will be deemed valuable acquisitions. Of course, it is hardly necessary to add, that all papers on the subjects of zoology in all its branches; of botany, mineralogy, geology, chemistry, electricity, galvanism, magnetism, and all mathematical communications connected with the subjects of natural philosophy, will be thankfully received, and always duly acknowledged.

The want of a sufficient incitement towards inquiries of this nature, after University students have commenced graduates, has been sometimes considered as a defect in the scheme of University education. At that important period of life, when the application of philosophical studies should begin, academical students seem to have acted under an impression that they have brought their studies to a termination. Or, if a disposition should prevail, to approach the studies of Nature, under the conviction that it is better "*de re ipsa quærere, quam mirari,**" this tendency, of such incalculable value in youthful minds, becomes checked, either by the retirement or consequent want of intercourse with literary men, to which the calls of professional duties consign them, or by the little honour which in all our Universities has hitherto awaited the inquiry. The valedictory observations of Bishop Watson afford a decisive confirmation of this truth:† and the reproaches cast upon our country by the celebrated Kirwan‡ may be still considered as not altogether inap-

* See Science. † Watson's Miscellaneous Tracts, vol. ii. p. 426. Lond. 1815.

‡ Min. Pref. p. 1. Lond. 1786.

plivable. "In Sweden and Germany," says he, "mineralogy is considered as a science worthy the attention of government. There are Colleges in which it is regularly taught; it forms a distinct and honourable profession, like that of the soldier, the merchant, or the barrister; its superior officers form a part of the administration of the state. Young students fraught with the knowledge to be acquired in their own country are sent abroad to glean all that can be collected from a more diversified view of nature. This example has been followed by France, Russia, and Spain. Chemistry too, the parent of mineralogy, is cultivated by the most enlightened nations in Europe, and particularly in France, with a degree of ardour that approaches to enthusiasm. In England, on the contrary, it receives no encouragement from the public." These observations which the eminent naturalist then applied to the studies in which he was more particularly engaged, may, to a certain extent, be yet directed towards every other branch of natural philosophy. In the posthumous works of Dr. Hooke, which were dedicated to Sir Isaac Newton, when he was President of the Royal Society, by its Secretary Waller,* we find their author maintaining, that the neglect shown to natural philosophy has been characteristic, not of this country alone, but of all nations and in all ages. "Learned men," he complains, "take only a transient view of natural philosophy in their passage to other things; thinking it sufficient to be able to talk of it in the phrase of the school. Nor is it only so now, but it has been so almost in all ages; so that for about 2000 years, of which we have some account in history; there is not above one quarter of that space in which men have been philosophically given; and among such, as have been so, several of them have been so far disjoined by time, language, and climate, by manner of education, manners and opinions, and divers other prejudices, that it could not be expected it should make any considerable progress."

Yet the effect of such studies upon the mind, and especially in places appropriated to public education, and in an age when false philosophy and irreligion have been so alarmingly manifested, may perhaps secure for them a more favourable reception; since it requires no argument to prove that the evidences of religion always keep pace, and are progressive, with the discoveries in natural knowledge. After a long life entirely devoted to the studies of natural history, Linnaeus placed over the lintel of the door of his museum an inscription which was calculated to convey to the mind of every approaching student a conviction of this truth; *Innocuè vivito! Numen adest!*†

Having thus set before the Society the main design and object

* Hooke's Present State of Natural Philosophy; see Posthumous Works, p. 4. Lond. 1705.

† See Linnaeus's Diary, written by himself, in Palaeony's *Linnaeus* by Swediaur, p. 165. March 1665.

of the Institution, the Council beg to call the attention of this meeting to considerations of a subordinate nature. It will be necessary to provide some place in which the future meetings may be held, and where a repository may be formed for the preservation not only of the archives and records of the Society, but also of such documents, books, and specimens, of natural history, as may hereafter be presented or purchased. The utmost economy will at present be requisite in the management of the Society's funds; and, therefore, if the consent of the University could be obtained, it would be highly desirable that the expenses of printing the Society's Transactions, should be defrayed by the University.* His Royal Highness the Chancellor has accepted of the office of Patron, and his letter, containing the expression of his approbation, will be read by one of the Secretaries. The present Vice-Chancellor, our High Steward, both our representatives in Parliament, and many other distinguished members of the University, who are not resident, have also contributed towards the undertaking; and there is, therefore, every reason to hope, that the Graduates of this University, who associated for the Institution of the Cambridge Philosophical Society, by their assiduity and diligence in its support, and by their conspicuous zeal for the honour and well-being of the University; will prove to other times, that their lives and their studies have not been in vain.

ARTICLE XVII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

1. Notice of some new Minerals from Finland. By M. Jahn, of Albo.

As any information contributing to promote the knowledge of minerals will probably be agreeable to your readers, I submit to your disposal a brief notice of an investigation, made by Mr. N. Nordenskiöld,† of the crystalline form and the chemical constituents of several Finnish minerals, among which the most recently discovered are samarskizit and pyralolit.

Samarskizit.—M. Nordenskiöld has named this new mineral after Count Romanzoff, who is well known to promote scientific pursuits in general, with unbounded liberality, and whose attention and generosity in particular, contribute to investigations relating to the mineralogy, language, and history of Finland.

* This is now done.

† Mr. Nordenskiöld is a Finland gentleman, a pupil of Berzelius. He published a paper on the samarskizit in a pamphlet, entitled, "Bilag till samarskizitens beskrifning af Finlands Mineralogi or Geognosi," from which this communication is taken.

This mineral is found in the lime quarry of Kulla, at Kimito, in Finland. The colours of this mineral are brown, brownish-yellow, and blackish-brown. It occurs compact, sometimes in crystalline planes, which indicate the rhomboidal dodecahedron with replaced edges; there seldom occur more than one perfectly crystallized plane with parts of the others, they incline at an angle of 120° to each other.

The fracture is small, conchoidal, splintery, and strongly resembles that of common resin.

The crystalline planes are highly splendid; the crystalline transition planes sometimes shining, sometimes dull; the lustre is greasy. When broken of the fragment, have a lustre between the vitreous and resinous; when thin, they are translucent.

Hard; brittle; give sparks with the steel: scratch glass and feldspar, but are scratched by quartz.

The specific gravity of this substance is 3.6096 at 60° Fahr. It is of light-yellow when powdered.

It melts without effervescence in the interior flame of the blow-pipe, giving a button of the same colour as the mineral, except when the flame is smoky when it is blackish.

Five grammes analyzed in the general way by fusing with carbonate of potash and dissolving in muriatic acid, yielded

Silex.....	41.24
Lime.....	24.76
Alumine.....	24.08
Oxide of iron.....	7.02
Magnesia and oxide of manganese.	0.92
Volatile parts, and loss.....	1.98

Neither the magnesia nor the oxide of manganese appear to belong to the chemical constituents. The oxygen in the lime is three times; the oxygen in the alumine five times, and the oxygen in the silex nine times the quantity of the oxygen in the oxide of iron, the mineralogical formula of course will be $= F S + 3 C S + 5 A S$, or $(F S + 2 A S, + 3 (C S + A S))$.

Pyrallolit.—A new mineral belonging to the talc family.

Among the minerals found in the lime-quarry of Storgard in the point of Pargas, there is one, which at first was considered to be crystallized talc. It has the singular propensity of blackening before the blow pipe at a low red-heat, and it afterwards becomes white at a higher temperature. It occurs in opaque sparry limestone accompanied with feldspar, augit, skapolit, moroxit, and sphene, and particularly crystallized with augit, which mineral often thinly covers it. His Excellency Count Steinheit who has examined the quarries of Parga with the greatest care, and to whose zeal the mineralogist is indebted for the discovery of most of the new Finnish minerals, was also the first who gave attention to this mineral.

This mineral is found in crystalline masses, and in distinct crystals of four varieties of form.

In quadrangular prisms, of which the angles are $94^\circ 36'$ and $85^\circ 24'$, and which are, therefore, slightly rhomboidal, the opposed lateral planes, two and two, differ in breadth; the plane M and its opposite plane (Plate V), fig. 9, being much broader than T and its opposite plane, on M $140^\circ 49'$.

Fig. 10 differs from fig. 9 only by the addition of the little planes π : n on T $131^{\circ} 30'$.

Fig. 11, P on l $129^{\circ} 11'$. The plane P is at right angles to the axis of the prism.

Fig. 12. In this, the planes π of fig. 10 are seen in combination. P and l of fig. 11. n on P $138^{\circ} 30'$.

The crystals seldom occur perfect; they vary in size from very small to the length of 1 and 2 inches, 3-10ths to 4-10ths of an inch in breadth. The colour is sometimes greenish. By long exposure to air and light, the coloured crystals become perfectly white. Their surface is dull. The lustre greasy. Fracture, dull-earthly. Translucent only when in thin laminae. The crystals are more or less coherent: some readily crumble, and are then unctuous to the touch; others are harder, yielding to the knife but not to the nail, and feel then harsh in the fracture. The same crystal is sometimes hard in one end, and not at the other. It seems to harden in the air. By fracture three-sided prisms are very easily obtained. Its specific gravity is 2.555 to 2.594.

The powder put upon a red-hot spoon phosphoresces with a bright bluish light.

When heated with the blow-pipe at a little below a red heat, it becomes blackish, and by continued exposure to this heat, it is rendered white, swells, and melts at the edges into a white enamel.

It melts with borax readily into a clear glass, which, by adding a little nitre shows traces of manganese. A little of the phosphate of soda, or ammonia added to a button of borax, saturated with the mineral, renders it after cooling an opaque and white enamel. A little piece of the mineral heated with glass of phosphorus effervesces slightly at first, but they eventually combine.

With soda it melts to a clear glass; with a yellowish-green tinge, the colour is most easily distinguished upon white paper.

The analysis, which was conducted in the usual mode, viz. by heating the fine powdered mineral with three times its weight carbonate of potash, dissolving it in muriatic acid, gave

Silica	56.62
Magnesia	23.38
Alumine	3.38
Lime	5.58
Protoxide of manganese	0.99
Peroxide of iron	0.09
Water	3.58
Bituminous matter and loss	6.38

100.00

To attempt a formula for this mineral before it is determined whether the bituminous matter belongs to the chemical composition or not would certainly be premature. But it may be observed that as the oxygen of the alumine and lime is equal, and that of the silica, 1.1 times, of the magnesia six times, of the water twice, the quantity of oxygen in the alumine, the formula is likely to be $AS^2 + CS + 4H$.

6 Mg. S² 4. 2. Aq. in case the bituminous matter is considered as foreign to the chemical composition.

In consequence of the singular property of this mineral of the change of colour when exposed to heat, from white to dark and from dark to white again, the name pyraloida, formed of the Greek words *πυρ* and *αλλας*, was given to it.

II. Mining Intelligence.

At the latter end of December last, the third of the new steam engines erected on the Consolidated mines near Redruth, in Cornwall, was put to work; and as two of these machines are of a larger size than was ever before attempted, and as the concern is one of great extent and interest, some account of it may be acceptable to our readers.

The undertaking includes four or five copper mines nearly adjoining, and on the same veins, formerly worked very profitably in distinct portions, until owing to difficulties in pumping the water, and other circumstances, they were gradually abandoned about 16 years since.

The improvement in the use of steam power since that period is a prominent reason for expecting advantage to those who have had the spirit to renew the workings, though there are many others which are important, such as increased skill in the management of mining processes, and a reduction in the cost of labour and those materials which are most largely consumed. The present company have engaged a capital of about £65,000. in the concern, and the whole is under the management of Capt. William Davey and Mr. John Taylor.

The extent of underground workings to be drained is very considerable, running for about a mile in length, and reaching at the lowest part to a depth of about 180 fathoms under the adit, or level by which the water is discharged towards the sea.

To keep the whole of these excavations dry, and to enable the mines to be sunk deeper, three engines have been erected by Mr. Arthur Woolf. One at the western extremity of the ground having a cylinder of 70 inches diameter, which works a pump about 60 fathoms deep, and two others, which we mean particularly to notice, are situated one near the centre, and the other at the eastern end of the concern.

These engines have cylinders of 90 inches diameter, the pistons make a stroke of 10 feet in the cylinder, and the centre of the beams is so fixed that the rods make an eight foot stroke in the pumps; thus they are able, at the common pressure, to raise a load of 85,000 lbs. Each of the engines is furnished with six wrought-iron boilers for producing high pressure steam, which is applied in the mode usually called expansive by engineers, and is condensed in the common manner.

Three boilers are connected so as to be heated by two fires, and are sufficient to work the engine, leaving three others to be applied when those which have been in use are cleansing or repairing.

These immense engines are executed in a very beautiful manner, and exhibit remarkable instances of accurate workmanship and sound calculation. Though they exceed in power all others that have been before constructed, and of course every part is of a dimension for which there is no precedent, yet each has, from the first, performed its office might, and the combination is so perfect that the motions are equal,

and from friction or concussion. The engines have worked repeatedly for days at the rate of 12 and 13 strokes a minute, and the whole has gone as smoothly as if a fly wheel regulated the impulse.

The effect of the first of these engines, or, as it is called in Cornwall, the duty, has been regularly calculated by the person appointed for that purpose, and has been published in the monthly report.

It was found to have consumed about 3,800 bushels of coal in 35 days, or 111 bushels per day, and the effect had been that of raising 3,500,000 lbs. of water by each bushel of coal, which is rather more than was done in the same period by any engine of similar construction.

It may be worth mentioning the weights of some of the principal parts of one of these large machines. The cylinder, exclusive of the cover and bottom, weighs about $12\frac{1}{2}$ tons, in one piece; it is surrounded by a case of still greater dimensions. The beam with its gudgeon weighs nearly 25 tons.

The pump rods in the shaft are the largest mast timber that could be procured, and are 16 inches square to a considerable depth in the shaft; when the whole are attached, they will weigh, with the iron plates which connect them together, nearly 40 tons.

When it is considered that to this latter weight is to be added that of the column of water, and one half of the beam, we shall find nearly 100 tons on one side the centre, and of course a corresponding pressure on the other side to counterpoise it; so that there is suspended on the gudgeon, and moving freely upon it, nearly 200 tons.

The piston frequently passes through 240 feet every minute, and gives a corresponding velocity of motion to this immense mass of matter, which is yet regulated with a precision that is astonishing, and which acts without concussion, and without disturbance, to the various parts of the machine.

There are many most ingenious improvements in the construction, and the arrangement is simple and complete. The whole reflects great credit on the skill and ability of Mr. Woolf, to whom Cornwall has before been indebted for the introduction of some of the most important improvements in steam engines, that have benefited the mines in later years.

The works at the Consolidated Mines were only commenced in Jan. 1849, and it is probable that in a few weeks the water will be all pumped out; so that this, with the extensive erections for various purposes on the surface, which are such as to render it probably the largest and most complete mining establishment in the world, have been executed in two years.

ARTICLE XVIII.

NEW SCIENTIFIC BOOKS

TEACHING FOR EDUCATION.

Mr. Leach has nearly completed his Synopsis of British Mollusca.
 Genl. Von Kotzebue's Narrative of a Voyage round the World, in the Russian ship Revric, is translating for the press.

Elementary Illustrations of the Celestial Mechanics of Laplace, comprehending the First Book, for Students, in the Mathematics, may be shortly expected. in 8vo.

Mr Wood has in the press the Linnean Genera of Insects, illustrated by 86 coloured plates, and general observations on each genus.

Dr. Forbes is about to publish his Observations on the Climate of Penzance, and the Districts of the Land's End, in Cornwall.

Dr. Henry Reader will shortly publish in 8vo. A Practical Treatise on Diseases of the Heart, in which will be comprised a full Account of all the Diseases of that Organ.

JUST PUBLISHED.

Peptic Precepts: pointing out Methods to prevent and relieve Indigestion, and to regulate and invigorate the Action of the Stomach and Bowels. 12mo. 3s. boards.

History and Method of Cure of the various Species of Palsy. By John Cooke, MD. 8vo. 6s. boards.

Monthly Journal of Popular Medicine. By Charles Haden, Surgeon. No. I. 1s. 6d.

Rome, in the Nineteenth Century, containing a complete Account of the Ruins of the Ancient City, the Remains of the Middle Ages, and the Monuments of Modern Times. 3 vols. post 8vo. 1l. 7s. boards.

A concise Account of the Origin and Principles of the new Class of 24-Pounder Medium Guns, of reduced Length and Weight, proposed in 1813 by Sir William Congreve, Bart. and adopted into his Majesty's Service. 8vo. 7s. 6d.

ARTICLE XIX.

NEW PATENTS.

John Sadler, of Penlington-place, Lambeth, for an improved method or process of manufacturing carbonate of lead, formerly denominated ceruse, but now commonly called white lead.—Jan. 3, 1821.

John Leigh Bradbury, of Manchester, for a new mode of engraving and etching metal rollers, used for printing upon woollen, cotton, linen, paper, cloth, silk, and other substances.—Jan. 9.

Robert Salmon, Esq. for improvements in the construction of instruments for the relief of hernia and prolapsis; which instrument, so improved, he denominates scientific-principled, variable, secure, light, easy, elegant, cheap, and durable trusses.—Jan. 15.

John Frederick Daniell, Esq. of Gower-street, Bedford-square, for improvements in clarifying and refining sugar.—Jan. 15.

Abraham Henry Chambers, Esq. of Bond-street, for an improvement in the manufacture of building cement, composition, stucco, or plaster, by means of the application and combination of certain known materials hitherto unused (save for experiments) for that purpose.—Jan. 15.

Charles Phillips, of Albemarle-street, commander in the royal navy, for improvements in the apparatus for propelling vessels, and improvements in the construction of vessels so propelled.—Jan. 19.

ARTICLE XX.

METEOROLOGICAL TABLE.

1821.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. ^s 9 a.m. t
		Max.	Min.	Max.	Min.			
1st Mon.								
Jan. 1	E	29.97	29.86	31	23	—		61
2	E	29.86	29.53	30	22	—		61
3	E	29.53	29.45	32	24	—		57
4	N E	29.50	29.45	31	25	—		56
5	E	29.45	29.27	37	29	—	—	60
6	E	29.28	29.25	41	33	—	—	88
7	N	29.28	29.26	39	34	—	—	86
8	E	29.26	29.09	45	34	—	70	89
9	E	29.15	29.04	43	37	—		96
10	S W	29.16	29.13	44	37	—	17	93
11	E	29.26	29.14	46	38	—	28	100
12	S	29.52	29.26	51	44	—	05	94
13	W	29.52	29.41	51	41	—	—	76
14	N E	30.12	29.41	42	30	—	1.20	80
15	S E	30.12	29.77	49	33	—	42	90
16	W	30.18	29.77	48	34	—		77
17	S W	30.25	30.18	46	39	—	—	91
18	S W	30.31	30.25	52	45	25	02	92
19	S W	30.32	30.31	48	40	—		92
20	W	30.60	30.32	50	28	—	05	83
21	Var.	30.61	30.60	44	28	—		70
22	N W	30.69	30.61	45	35	—		73
23	N E	30.70	30.67	40	27	—		80
24	S E	30.67	30.60	36	28	—		91
25	S W	30.60	30.50	48	34	—		94
26	N E	30.58	30.44	45	34	—		77
27	E	30.44	30.29	38	32	—		78
28	S	30.29	30.28	35	30	—		84
29	S E	30.29	30.20	45	30	—		90
30	S W	30.36	30.22	50	42	—		94
31	S W	30.38	30.36	51	44	35		84
		30.70	29.04	52	22	60	2.89	100—56

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

First Month.—1. A strong cold wind. 2—4. Cloudy: bleak. 5. Morning fine: about two inches of snow in the evening, followed by hail and rain, which thawed nearly all of it before morning. 6. Cloudy: the thaw continuing. 7. Cloudy. 8. Fine: cloudy. 9. Foggy. 10. Foggy: cloudy. 11. Rainy. 12. Rainy: fine at intervals. 13. Cloudy: very rainy night. 14. Rainy day: a lunar corona in the evening. 15, 16. Cloudy: fine at intervals. 17—20. Cloudy. 20. Cloudy: a lunar corona in the evening surrounded by a large halo. 21. Foggy morning: very fine day. 22. Gloomy: fine. 23. Fine. (The observation on the barometer here given is from the one which is constantly registered. Two others, probably less perfectly adjusted, were found to stand respectively at 30.85 and 30.95 inches; the clock barometer at Tottenham gives the maximum at about 30.78 inches; the whole at noon on the 23d.) 24. Gloomy: foggy. 25. Ditto. 26. Cloudy. 28. Ditto. 25. Foggy: misty. 29. Fine clear morning: fine day. 30, 31. Fine.

RESULTS.

Winds: N, 1; NE, 4; E, 9; SE, 3; S, 2; SW, 7; W, 3; NW, 1; Var. 1.

Barometer: Mean height

For the month..... 29.939 inches.

For the lunar period, ending the 26th..... 29.837

For 13 days, ending the 8th (moon south)..... 29.668

For 14 days, ending the 22d (moon north)..... 29.893

Thermometer: Mean height

For the month..... 38.172°

For the lunar period..... 36.363

For 30 days, the sun in Capricorn..... 35.616

Evaporation..... 0.60 in.

Rain..... 2.89

Mean of hygrometer..... 81°

Laboratory, Stratford, Second Month, 20, 1821.

R. HOWARD.

ANNALS OF PHILOSOPHY.

APRIL, 1821.

ARTICLE I.

Experiments to determine the true Weight of the Atoms of Copper, Zinc, Iron, Manganese, Nickel, and Cobalt. By Thomas Thomson, MD. FRS.

IN two papers published in vol. xvi. of the *Annals of Philosophy*, p. 327, and in the first number of the New Series, I have shown by experiments, which to me at least appear decisive, that the atomic weights of the 14 bodies which constitute the subject of these papers are as follows :

	Oxygen = 1.	Hydrogen = 1.
Nitric acid.	6.75	54
Sulphuric acid.	5.0	40
Muriatic acid.	4.625	37
Chromic acid.	6.5	52
Arsenic acid.	7.75	62
Phosphoric acid.	3.5	28
Potash.	6.0	48
Soda.	4.0	32
Barytes.	9.75	78
Strontian.	6.5	52
Lime.	3.5	28
Magnesia.	2.5	20
Silver.	13.75	110
Lead.	13.00	104

The process which I followed succeeds also with the sulphates.
New Series, VOL. I.

of the metals, whose names are affixed to the title of this essay, and has enabled me to determine their atomic weights with equal precision; but with mercury, bismuth, and tin, some modifications will be requisite. The difficulty with respect to mercury is to obtain its nitrate perfectly free from water. The chlorides of tin and of bismuth are easily procured in a state of dryness; but when thus freed from water, I could neither dissolve them completely in nitric acid, acetic acid, or water; and the addition of muriatic acid destroys the precision of the process.

My object at present is to relate the experiments which I have made to determine the weights of the atoms of the six metals whose names are enumerated in the title to this essay. The following table exhibits the atomic weights of these bodies according to the best experiments hitherto published.

	Dalton.*	Wollaston.†	Berzelius.‡	Prout.§	Thomson.
Copper.	7.0	4.0	7.9139	4.0	8.0
Zinc.	7.0	4.1	8.0645	4.0	4.25
Iron.	6.25	3.45	6.7843	3.5	3.5
Manganese	5?	—	7.1157	7.115	3.5
Nickel.	3.125? or 6.25?	—	7.3951	3.65	3.25
Cobalt.	6.875?	—	7.3800	3.66	3.25

The numbers given by Berzelius being alone (except my own), deduced from original experiments, it will be worth while to compare them with mine; but to fit them for that comparison, it will be requisite to divide all of them, except the first, by 2. This gives us their weights as follows:

Copper	7.91390
Zinc	4.03225
Iron	3.39215
Manganese	3.55785
Nickel.	3.69755
Cobalt	3.69000

The long train of decimals attending almost every one of these numbers is sufficient of itself to render their accuracy suspicious. Nature is not wont to indulge in such complicated relations. Indeed the very simple numbers, which we have already obtained for the atomic weights of the 14 bodies formerly examined, lead almost irresistibly to the conclusion that the same simplicity will be found to pervade the weights of all the simple bodies in nature. We shall see immediately at least that the law holds

* New System of Chemistry, p. 546. I have divided his number by 8.

† Phil. Trans. 1814, p. 18.

‡ Essai sur la Théorie des Proportions Chimiques. Table at the end.

§ *Annals of Philosophy*, vi. 329. Most of his numbers are deduced from Berzelius's experiments.

most admirably as far as the bodies at present under our review are concerned.

I. Copper.

It will be seen from the preceding table that Dr. Wollaston and Dr. Prout consider the weight of an atom of copper to be 4, while I make it 8. Both the numbers of these gentlemen and my own are deduced from the sulphate of copper, or blue vitriol, the crystals of which, by a careful analysis, I have found composed as follows :

Sulphuric acid	10·00
Peroxide of copper	10·00
Water.	11·25
	<hr/>
	31·25

From this it appears that the salt contains equal weights of acid and base, and that 9-25ths of its weight is water. Now the weight of an atom of sulphuric acid being 5, it is obvious that if we consider blue vitriol a compound of one atom acid and one atom base, an atom of peroxide of copper will weigh 5 (exactly the same as sulphuric acid), and consequently an atom of copper will weigh 4. This is the reasoning which led Wollaston and Prout to fix upon 4 for the atomic weight of copper.

On the other hand, I am of opinion that blue vitriol is a bisulphate of copper, or a compound of two atoms of sulphuric acid and one atom of peroxide of copper, and that its composition may be thus stated :

2 atoms sulphuric acid.	=	10·00
1 atom peroxide of copper	=	10·00
10 atoms water	=	11·25
		<hr/>
		31·25

If this constitution be admitted, it is obvious that an atom of peroxide of copper must weigh 10, and consequently an atom of copper 8. Now the reasons that lead me to conclude that blue vitriol is a bisulphate of copper are the following :

(1.) It is well known that copper forms two oxides with oxygen, the red and the black, and that the oxygen in the black oxide is precisely double that in the red oxide. Hence I think it likely that the protoxide is a compound of one atom copper + one atom oxygen, and the peroxide of one atom copper + two atoms oxygen. If this be admitted, an atom of copper must weigh 8, and the composition of the oxides will be as follows :

Protoxide	8 copper + 1 oxygen
Peroxide	8 + 2

Dr. Wollaston and Dr. Prout must consider the black oxide

of copper as a compound of one atom copper + one atom oxygen, and the protoxide as a compound of two atoms copper + one atom oxygen; so that their constitution, according to this view of the subject, is as follows :

Protoxide.	8 copper + 1 oxygen
Peroxide	4 + 1

I consider my view of the subject as more simple and more conformable to the analogy of other metals.

(2.) Blue vitriol, how frequently soever we crystallize it, or how carefully soever we wash it, always reddens vegetable blues, and of course contains an excess of acid. Now most of the salts containing two atoms of acid united to one atom of base that I am acquainted with, have the property of reddening vegetable blues. This is the case with

Bisulphate of potash,
Bisulphate of soda,
Bitartrate of potash,
Binoxalate of potash,
Alum,

and various other bisalts which will readily occur to the recollection of the practical chemist. Blue vitriol then possesses the characters of a bisalt. Hence it is reasonable to conclude that it is a compound of two atoms of acid and one atom of peroxide.

(3.) There exist other two sulphates of copper besides blue vitriol, both of which I have repeatedly obtained.

If you dissolve 31.25 grs. of the crystals of blue vitriol in water, put into the solution 10 grains of peroxide of copper in the state of a fine powder, and digest the whole in a retort on a sand-bath for two months, the whole of the oxide gradually disappears, the solution becomes colourless, and a green coloured powder is deposited, which is a neutral sulphate of copper; for it contains, as is obvious, twice as much peroxide of copper as blue vitriol. This neutral sulphate has no effect on vegetable blues, and is insoluble in water; but it dissolves easily in muriatic acid. If we analyze it, we shall find it to consist of one sulphuric acid + two peroxide of copper by weight, and a certain quantity of water, which I have not determined with accuracy.

If into a solution of blue vitriol you pour a quantity of sulphuric acid, and evaporate the liquid sufficiently in a Wedgewood dish, a salt is deposited in small irregular crystals having a much lighter blue colour than blue vitriol, a much more acid and acrid taste, and acting more powerfully on vegetable blues. This salt is slowly deliquescent, and from the few trials to which I have subjected it, I have reason to consider it as a perquadrissulphate of copper, or a compound of four atoms of sulphuric acid and one atom of peroxide of copper.

The existence of these three salts, similar to the oxalate, binoxalate, and quadroxalate of potash, and in some respects similar to the triple oxalates of potash, soda, ammonia, and copper, discovered and analyzed by Vogel, of Bareuth, leave little doubt in my mind that the true atomic weight of copper is 8.

(4.) The common green permuriate of copper is a compound of one atom peroxide of copper and two atoms of muriatic acid; for when 10 grs. of peroxide of copper are dissolved in muriatic acid, and the liquid evaporated to dryness, a brownish yellow coloured mass remains, which weighs exactly 17 grs. It is, therefore, a perchloride of copper composed of

$$\begin{array}{rcl} 1 \text{ atom copper.} & \dots\dots\dots & = 8 \\ 2 \text{ atoms chlorine.} & \dots\dots\dots & = 9 \\ & & \hline & & 17 \end{array}$$

There exists likewise a protochloride of copper, described by Boyle under the name of rosin of copper, and since particularly described by Proust, Chenevix, and by Dr. Davy who analyzed it, and showed it to be a compound of

$$\begin{array}{rcl} 1 \text{ atom copper.} & \dots\dots\dots & = 8\cdot0 \\ 1 \text{ atom chlorine} & \dots\dots\dots & = 4\cdot5 \\ & & \hline & & 12\cdot5 \end{array}$$

(5.) There is reason to conclude from the experiments of Berzelius, that sulphuret of copper is a compound of

$$\begin{array}{rcl} \text{Copper.} & \dots\dots\dots & 8 \\ \text{Sulphur.} & \dots\dots\dots & 2 \\ & & \hline & & 10 \end{array}$$

So that an integrant particle of it weighs precisely as much as the peroxide of copper. Now it is well known that an atom of sulphur weighs exactly twice as much as an atom of oxygen. Hence the sulphuret must be a compound of one atom sulphur + one atom copper; and if so, an atom of copper must weigh 8.

I shall now proceed to show that the composition of blue vitriol is exactly as I have stated in a preceding part of this essay; namely,

$$\begin{array}{rcl} 2 \text{ atoms sulphuric acid.} & \dots\dots\dots & = 10\cdot00 \\ 1 \text{ atom peroxide of copper.} & \dots\dots\dots & = 10\cdot00 \\ 10 \text{ atoms water} & \dots\dots\dots & = 11\cdot25 \\ & & \hline & & 31\cdot25 \end{array}$$

The reader, by turning to a former paper of mine (*Annals of*

246 *Dr. Thomson's Experiments to determine the Weight* [APRIL, *Philosophy*, xvi. 330), will find that chloride of barium is composed of

1 atom chlorine.	= 4.5
1 atom barium.	= 8.75
	<hr/>
	13.25

And that when this salt is dissolved in water, it becomes muriate of barytes composed of

1 atom muriatic acid.	= 4.625
1 atom barytes.	= 9.75
	<hr/>
	14.375

Dissolve in a minimum of distilled water 31.25 grs. of blue vitriol, quite pure, dry, and in crystals. In another vessel dissolve $13.25 \times 2 = 26.5$ grs. of chloride of barium. Mix the two solutions together, and allow the mixture to stand till the precipitate has fallen down, and the supernatant liquor is quite clear. Examine this liquor by mixing it with muriate of barytes and with sulphate of soda in two separate vessels. If the experiment has been properly conducted, the liquid will not be in the least altered by either of these reagents, showing clearly that it contains neither sulphuric acid nor barytes. Hence it is clear that the quantity of sulphuric acid (two atoms) in 31.25 grs. of blue vitriol is just saturated by the quantity of barytes (two atoms) in 26.5 of chloride of barium. If, instead of 31.25 grs. of blue vitriol, you take only 31 grs. or even 31.125 grs. the liquid will be found to contain an excess of barytes.

If 31.25 grs. of blue vitriol be dissolved in water, and a plate of zinc put into the solution, the copper will be precipitated, and it will be found to weigh exactly eight grains.

II. Zinc.

Many attempts have been made by chemists to determine with precision the atomic weight of zinc. Those of Berzelius and my own will be found in an early volume of the *Annals of Philosophy*. I had deduced from these experiments 4.125 as the weight of an atom of zinc. Dr. Wollaston's weight, 4.1, may be considered as nearly the same with mine. Dr. Prout fixed the weight at 4, chiefly from theoretical considerations. The following experiments will show that all these determinations are under the truth.

Sulphate of zinc, or white vitriol, as it was formerly called, is a salt, which crystallizes in four-sided prisms, and is transparent. It may be freed from its water by a cautious exposure to a red heat; and 17 grs. of the crystals, when thus treated, lose exactly 6.75 grs. Of course the residual salt weighs 10.25 grs.

Dissolve 17 grs. of crystals of sulphate of zinc in water.

Dissolve in another portion of that liquid 13·25 grs. of pure chloride of barium. Mix together the two solutions, and let the precipitate subside. The clear supernatant liquid is neither affected by muriate of barytes, nor by sulphate of soda; consequently it neither contains sulphuric acid nor barytes. It is obvious from this that 17 grs. of sulphate of zinc contain exactly five grs. of sulphuric acid. These facts warrant the conclusion, that the composition of crystallized sulphate of zinc is as follows:

1 atom sulphuric acid	= 5·00
1 atom oxide of zinc.	= 5·25
6 atoms water	= 6·75
	<hr/>
	17·00

Thus it appears that an atom of oxide of zinc weighs 5·25; consequently zinc itself must weigh exactly 4·25.

It may, perhaps, be objected that my method of determining the quantity of water in the salt might not be absolutely correct, as I had no means of determining whether the residual salt retained any water or not. But let it be remembered that the sulphuric acid in 17 grs. of the salt is exactly five grains, that the oxide of zinc certainly approaches five in weight, and that if we suppose the water to be increased by another atom, its weight would amount to 7·875 grs. leaving only 4·125 for the weight of an atom of oxide of zinc, which we are perfectly certain from preceding experiments is greatly below the truth. It cannot then be doubted that the atom of oxide of zinc weighs 5·25; consequently the weight of the atom of zinc must be 4·25, or a very little more than I had concluded from the previous experiments of Berzelius and myself.

III. Iron.

It is well known that iron unites with two doses of oxygen, and forms two oxides, the black and the red; the oxygen in which bear to each other the ratio of 2 to 3. This led at first to the supposition that the black oxide of iron was a compound of one atom iron + two atoms oxygen, and the peroxide of one atom iron + three atoms oxygen. It was under this impression that Mr. Dalton fixed the weight of an atom of iron at $6\frac{1}{4}$; but when we examine the salts of iron, we do not find them in accord with this view of the subject. Thus sulphate of iron (abstracting the water) is a compound of 5 sulphuric acid + 4·5 black oxide of iron. Now if we dissolve 9·5 grs. of anhydrous sulphate of iron in water, and 20·75 of dry nitrate of lead in another portion of water, and mix the two liquids together, we find that two new salts are formed, both of which are neutral; namely, sulphate of lead, and nitrate of iron; so that 4·5 grs. of black oxide of iron just neutralize 6·75 grs. of nitric acid. We can have no doubt from this that an atom of protoxide of iron weighs 4·5, or nearly

248 *Dr. Thomson's Experiments to determine the Weight* [APRIL, 30. It was the knowledge of these facts that led Dr. Wollaston to pitch upon 3·45, and Dr. Prout upon 3·5, as the atomic weight of iron.

I find by a very careful analysis, that the crystals of protosulphate of iron are composed as follows :

1 atom sulphuric acid.	= 5·0
1 atom protoxide of iron.	= 4·5
7 atoms water.	= 7·875
	<hr/>
	17·375

For 17·375 grs. of this salt, when dissolved in water and precipitated by muriate of barytes, give 14·75 grs. of sulphate of barytes, indicating five grains of sulphuric acid. When precipitated by ammonia, and the precipitate well dried, it weighs five grs. of peroxide of iron, which is equivalent to 4·5 grs. of protoxide of iron. The remaining 7·875 grs. must be the water of crystallization. This view of the subject is confirmed by the following experiment :

I dissolved 17·375 grs. of the crystals of protosulphate of iron in distilled water : 13·25 grs. of chloride of barium were dissolved in another portion of water, and the two liquids were mixed. After the precipitate had subsided, the residual liquid was neither affected by muriate of barytes, nor sulphate of soda. It is obvious, therefore, that it contained neither sulphuric acid nor barytes ; consequently 17·375 grs. of crystallized protosulphate of iron contain exactly five grs. of sulphuric acid. The remaining 12·375 grs. consist of protoxide of iron and water. It may, perhaps, be objected to the determination of the weight of an atom of protoxide of iron at 4·5 from this experiment, that we are not absolutely certain that the portion of water in 17·375 grs. of these crystals is exactly 7·875 grs., or 7 atoms ; but the facts ascertained are perfectly decisive on this point. By the analysis of Berzelius, the water in 17·375 grs. of protosulphate of iron weighs 7·888 grs. By my analysis, it weighs 7·819 grs. Now the mean of these two numbers is 7·853 grs. which is only $\frac{1}{1000}$ th of a grain less than the theoretical quantity ; but if the water in this salt were 8 atoms instead of 7, its weight would amount to 9 grs. instead of 7·875 ; and only 3·375 grs. would remain for the oxide of iron ; but we are perfectly certain that its weight greatly exceeds that quantity. It is evident that the quantity of water in the salt must be a whole number of atoms : 7·875 being the weight of seven atoms of water, and the liquid amounting sensibly to that quantity as nearly as can be estimated by experiment, we are entitled to consider it as the exact quantity ; consequently an atom of protoxide of iron must weigh 4·5. This gives 3·5 for the weight of an atom of iron, almost the number pitched upon by Wollaston, and the very

1821.] of the Atoms of Copper, Zinc, Iron, Manganese, &c. 249
 number made choice of by Prout. Berzelius's number is certainly too small.

It deserves to be mentioned that the whole of the iron cannot be precipitated from a solution of protosulphate of iron by means of phosphate of soda, in what proportion soever the saline solutions are mixed. The supernatant liquid contains both oxide of iron and phosphoric acid; for it strikes a blue with prussiate of potash, and is precipitated white by nitrate of lead. Indeed I know no neutral salt capable of throwing down the whole of the iron from a solution of this salt.

It is not very easy to procure protosulphate of iron perfectly free from peroxide of iron. The best way seems to be to dissolve the crystals in hot water, to filter the solution, and impregnate it with sulphuretted hydrogen gas. The crystals formed in such a liquid are pure protosulphate of iron. The mark by which the purity of this salt, or at least its freedom from peroxide of iron, may be recognized is, that prussiate of potash precipitates it white instead of blue.

IV. *Manganese.*

The only person who has endeavoured to determine the weight of an atom of manganese experimentally before me is Berzelius; and his determination (when reduced) differs but little from mine; for 3·55785, though a complicated number, exceeds 3·5 only by a very small fraction. I shall describe the experiment which I consider as demonstrating that the true weight of an atom of protoxide of manganese is 4·5. Sulphate of manganese is easily obtained by dissolving carbonate of manganese in sulphuric acid, and evaporating the solution sufficiently. It forms beautiful rhomboidal flesh-coloured prisms which are not sensibly altered by exposure to the air. After a few preliminary trials on this salt, I concluded that it is a compound of

1 atom sulphuric acid.	= 5·0
1 atom protoxide of manganese. ..	= 4·5
5 atoms water.	= 5·625
	<hr/>
	15·125

To verify this conclusion, I dissolved 15·125 grs. of the dry crystals in water; 13·25 grs. of dry chloride of barium were dissolved in another portion of water; and the two liquids were mixed together. After the precipitate of sulphate of barytes had subsided, the supernatant liquid was examined by means of muriate of barytes and sulphate of soda. It was not affected by either of these reagents. It follows from this, that 15·125 grs. of sulphate of manganese contain exactly 5 grs. of sulphuric acid. The remaining 10·125 grs. consist of water and protoxide of manganese. Now Dr. John obtained from 15·125 grs. of sulphate of manganese 5·345 grs. of water. I myself, from the same quantity of salt, extracted 5·48 grs. of water. John's

250 *Dr. Thomson's Experiments to determine the Weight* [APRIL, quantity is only one-fourth of an atom of water, and mine only one-eighth of an atom of water, less than 5.625 grs. But as neither three-fourths nor seven-eighths of an atom of water can enter into combination, we are entitled to consider the true quantity of water present as 5 atoms, or 5.625 grs.; consequently, the protoxide of manganese present must weigh exactly 4.5 grs. Thus we get 4.5 for the weight of protoxide, and 3.5 for the weight of manganese in the metallic state.

Thus we see that the atomic weights of iron and manganese are the same.

V. Nickel.

I am not certain that any chemist has taken the trouble to make an accurate analysis of sulphate of nickel. It is a salt which crystallizes in beautiful four-sided prisms, and is not altered by exposure to the atmosphere. I find that 17.125 grs. of this salt, when dissolved in water, and mixed with a solution of 13.25 grs. of chloride of barium, let fall sulphate of barytes; and the clear liquid, after the precipitate has fallen to the bottom, is neither affected by muriate of barytes, nor by sulphate of soda; consequently, it neither contains sulphuric acid nor barytes. It is clear from this, that 17.125 grs. of the salt contain exactly 5 grs. of sulphuric acid. When 17.125 grs. of crystallized sulphate of nickel are exposed to a red heat, they sustain a loss of weight varying from 7.8 grs. to 8 grs. Now the weight of 7 atoms of water is 7.875, which agrees almost exactly with the loss of weight found by experiment. It is obvious from this that 17.125 grs. of this salt are composed as follows:

1 atom sulphuric acid.	= 5.0
1 atom protoxide of nickel.	= 4.25
7 atoms water.	= 7.875
	<hr/>
	17.125

Thus it appears that protoxide of nickel weighs 4.25; consequently the weight of an atom of nickel must be 3.25.

VI. Cobalt.

The difficulty of obtaining this metal in a state of purity has hitherto retarded the exact determination of its atomic weight. I took a quantity of oxalate of cobalt, which I had purified by Laugier's process, exposed it to a red heat in a platinum crucible, and then dissolved it in sulphuric acid. By the requisite concentration, the sulphate of cobalt is obtained in small red prismatic crystals, which are not sensibly altered by exposure to the air. They may be exposed to an incipient red heat without losing any of their acid, provided we take care to raise the heat slowly, and not to carry it too far. By this method I was able, after a little practice, to deprive them entirely of their

water of crystallization, while they still continued completely soluble in water, and of course had lost no sensible quantity of their acid. After various trials I found that 9.25 grs. of this dry salt, when dissolved in water and mixed with a solution of 13.25 grs. of dry chloride of barium, left a liquid which was neither affected by muriate of barytes, nor sulphate of soda, and of course contained neither sulphuric acid nor barytes; therefore 9.25 grs. of dry sulphate of cobalt contain 5 grs. of sulphuric acid; consequently, the anhydrous sulphate must be a compound of

1 atom sulphuric acid.	= 5.0
1 atom protoxide of cobalt.	= 4.25
	<hr/> 9.25

Protoxide of cobalt then weighs 4.25. Hence an atom of cobalt must weigh 3.25.

We see that cobalt and nickel have exactly the same weight. From the table at the beginning of this paper, we see that Berzelius gives the weights of these two metals almost the same, viz.

Nickel	3.69755
Cobalt	3.69000

But these atomic weights are considerably higher than the truth.

From the preceding experiments, I consider myself warranted to conclude, that the atomic weights of the six metals which have occupied our consideration in this paper are as follows :

	Oxygen = 1.	Hydrogen = 1.
Copper	8.0	64
Zinc	4.25	34
Iron	3.5	28
Manganese	3.5	28
Nickel	3.25	26
Cobalt	3.25	26

Thus it appears that these six metals, like the 14 other bodies previously subjected to examination, have atomic weights which are multiples of hydrogen: so that if we reckon hydrogen by unity, they are all whole numbers. If we inspect the table of atomic weights when oxygen is unity, we may observe that the atomic weight is either a whole number, or a whole number together with one or other of the three following fractions, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{3}{8}$. The atomic weights would still continue multiples of the atom of hydrogen (0.125), though they consisted of numbers terminating in the fractions $\frac{1}{8}$, $\frac{3}{8}$, $\frac{5}{8}$, $\frac{7}{8}$: but these fractions seem to be excluded, at least as far as our observations have yet gone. The consequence of this is, that when the atom of hydrogen is unity, the atomic weights of all the other simple bodies hitherto exa-

mined are even numbers. It is obvious that this rule cannot apply to those atoms of compound bodies which contain hydrogen as a constituent. Thus the weight of an atom of water is 1.125, of an atom of muriatic acid 4.625, of an atom of ammonia 2.125. In the tables inserted at the end of the last edition of my System of Chemistry many exceptions to this law occur. The reason of this is, that though aware of the law, I did not choose to alter any of the atomic weights previously determined till I had satisfied myself, by unexceptionable experiments, that the old numbers are erroneous. I have little doubt that the atoms of bismuth, tin and antimony, and aluminum, are inaccurate, though I have not hitherto hit upon any mode of determining the true atomic weights of these bodies, which is quite free from ambiguity. The weight of bismuth, instead of being 8.875, as I have given it, is either 9, or 9.25; I am not quite sure at present which of the two; but as soon as I have obtained satisfactory results for these, and the other simple bodies hitherto remaining undetermined, I shall lay them before the public.

ARTICLE II.

Analysis of a Native Carbonate of Magnesia from the East Indies. By William Henry, MD. FRS. &c.

(To the Editor of the *Annals of Philosophy*.)

Manchester, March, 1821.

I WAS indebted to the kindness of Dr. Babington for a specimen of this mineral. He informed me that it was brought by his son from the East Indies; but as he has given me no account of its geographic situation or geognostic relations, I can only describe its external characters, and state the results of its chemical analysis.

It occurs massive. Its colour is snow-white, with the exception of a few small dots and stripes of ochre yellow. Its fracture is small conchoidal passing into uneven. It gives sparks with steel, and is not easily scraped by a knife, but is not hard enough to scratch fluor spar. Its fragments are sharp edged. Internally it has no lustre. It is very slightly translucent, and that only at the edges. Its specific gravity is 2.5615.

It dissolves in acids at ordinary temperatures with extreme slowness, even when finely powdered, and whatever may be the specific gravity of the acid; but by heat its solution is quickened, and carbonic acid is disengaged. This property is an obstacle to the determination of its proportion of carbonic acid, which cannot well be learned in the common way. To ascer-

tain it, I passed a fragment, weighing a few grains, into a jar inverted over mercury, and let up a sufficient quantity of dilute sulphuric acid, the full action of which required more than a week. Carbonic acid was disengaged at the rate of 1.1 cubic inch from every grain of the stone; and reckoning 100 cubic inches of carbonic acid at a mean temperature and pressure = 46.5 grs. 100 grs. of the mineral would yield 51 grs. very nearly of the acid gas.

One hundred grains, reduced to powder and calcined in a sufficient heat, lost 51.5 grs. A similar quantity being heated to redness at the bottom of a long glass tube, the upper and cool part of the tube just exhibited a visible degree of moisture, which could not amount to nearly a grain in weight.

One hundred grains were dissolved in heated and diluted sulphuric acid, with the exception of 1.5 gr. The insoluble part was not acted upon by being boiled to dryness with nitromuriatic acid, or with solution of potash, and when examined by a magnifier seemed to me to be a fine siliceous sand. The solution being evaporated, and the product heated to redness, gave 140 grs. of dry sulphate of magnesia, equivalent at 33 per cent. magnesia to 46 grs. of that earth.

One hundred grs. dissolved by an excess of nitromuriatic acid, left the same proportion as before of insoluble matter. The solution was evaporated to dryness, redissolved in water, and part of the magnesia which had separated was again taken up by muriatic acid, carefully added, so as to be very slightly in excess. From one half of the solution, subcarbonate of potash precipitated, at a boiling temperature, carbonate of magnesia, which, when washed, dried, and calcined, gave 23 grs. of the pure earth, equal to 46 grs. of magnesia, from 100 of the stone. The other half afforded, with a mixture of phosphate and carbonate of ammonia, a precipitate which, after being heated to redness, left 60 grs. of phosphate of magnesia, equivalent (if the base in 100 grs. of that salt be reckoned at 38.5 grs.) to 25 grs. of magnesia, or to 46 from 100 of the stone, thus confirming the other methods of analysis.

The absence of lime and alumine was inferred from the non-action of the proper tests of those earths on a solution of the stone in muriatic acid, rendered just neutral by ammonia. I am aware, however, that a minute proportion of lime, existing in any solution along with a considerable proportion of magnesia, is not discoverable by the usual tests. Of this, any person may be satisfied by mixing a saturated solution of sulphate of lime with an equal bulk of solution of sulphate of magnesia; for the mixed solutions are not rendered turbid by oxalate of ammonia, either immediately, or on standing. It is possible, therefore, that the mineral may contain a very small proportion of lime, though I did not discover any by analysis. The muriatic solution showed, on applying the proper tests, a minute trace of iron.

From the experiments which have been described, the mineral under examination may be pronounced to be an almost pure carbonate of magnesia, composed in 100 grains of

Magnesia.	46·0
Carbonic acid	51·0
Insoluble matter	1·5
Water	0·5
Loss	1·0
	<hr/>
	100·0

The acid and base are in atomic proportion, and the stone is constituted of one atom of each ; for as 46 to 51, so very nearly is 24·6 to 27·5, the numbers which, on Dr. Wollaston's scale, denote the equivalents of magnesia and carbonic acid.

I avail myself of this opportunity of expressing my doubt whether such a salt as the *bicarbonate of magnesia*, constituted of one atom of base and two atoms of acid, really exists. This doubt, it is true, is founded on the examination of a single specimen of a salt prepared by myself several years ago. It was formed by mixing a dilute solution of sulphate of magnesia with a solution of carbonate of soda highly charged with carbonic acid under pressure. No immediate change occurred ; but after a few days, small hemispherical masses, consisting of congeries of crystals radiating from a common centre, had formed on the inner surface of the vessel. Having a few of these left, I have lately again examined them, and find them composed of base 29, acid 30, and water 41. These proportions of acid and base, perhaps from some trifling impurity of the salt (on which, having now exhausted my whole stock, I cannot make any more experiments) do not exactly agree with the carbonate ; but they are very remote from those that should constitute a *bicarbonate*. Should any of your readers have prepared and analyzed a true bicarbonate of this earth, the information will, perhaps, be acceptable to others as well as to myself.

ARTICLE III.

On the Red Rock Marle, or Newer Red Sandstone.

By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

Bath Easton, Feb. 15, 1821.

If the following remarks on one of the British strata, which has as yet been but imperfectly described, appear to you worthy of insertion, they are much at your service. Some parts of the

detail may, perhaps, be thought too minute, but it would, I confess, be my own wish, had I the power and opportunity to render them yet more so. It is even at this day only by the diligent accumulation of facts (and not unfrequently of those which may seem at first the most insulated and insignificant) that we can hope to obtain materials for the accurate history of the globe which we inhabit. Believe me, my dear Sir,

Yours, with much esteem,

J. J. CONYBEARE.

The series of strata distinguished by geologists as the red rock marle, or newer red sandstone, occupies, as is well known, a considerable tract in the county of Devon. Its geological character and relations have been noticed by Deluc and others, and more than one opinion has been given as to the nature and origin of its contents, which, with a most sincere respect for those from whom they have proceeded, I can scarcely regard as compatible with the results of a diligent, though, perhaps, a too partial examination. A short residence at Dawlish afforded me the opportunity, as well of observing the various aspects of the rock itself, which, on that part of the coast, offers large and uninterrupted sections, as of forming a tolerably complete series of the different substances imbedded in the strata which extend from thence to Teignmouth.

In these strata the rock exhibits itself under the several characters of a sandstone, either loosely compacted, or altogether pulverulent; a marle, more or less indurated; and a breccia composed of fragments of various sizes. Near to Dawlish, the sandy form is more frequent, towards Teignmouth the breccia, the base of which is usually marle, of an unctuous and argillaceous character. The marle has frequently those patches of white and purple, which have been often noticed as characteristic. The mineral contents of the rock seem to be few. *Calcareous spar* occurs in small patches a little south of Dawlish. *Gypsum* I could not detect either here or under the elevated plains of Haldon. On Blackdown, however, I have found it in small nodules. A sand sufficiently charged with, and indurated by iron, to be termed *ironstone*, traverses in all directions, the cliff to the north of Dawlish (see Deluc, vol. ii. p. 85), and the *earthy brown oxide of manganese* is found in numerous small cavities nearly through the whole range of the coast. In one instance I detected a small portion of the black oxide of cobalt, precisely resembling that found at Alderley edge (Cheshire) in the same strata.* But the most remarkable feature in the rock appears to

* I am indebted to the Rev. the Dean of Bristol for an addition to this meagre catalogue. That gentleman has had the kindness to forward me some specimens of chalcodony, which he discovered in this rock not far from Torquay. It is coarse, and appears under the form of spherical nodules either hollow or investing portions of the marle itself

be the variety of substances contained in its brecciated form. Deluc has mentioned two only of these, the limestone and greywacke, though he insinuates that there are others, and appears (if I rightly understand the paragraph) to apprehend that of these the limestone only can be traced to any neighbouring rock, an opinion which, in its full extent, seems untenable. The following catalogue presents a tolerably faithful description of the fragments occurring in the breccia of Dawlish and Teignmouth, with the addition of some few from other quarters.

Granite and Porphyritic Rocks.—These form a very considerable portion of the imbedded fragments. A¹. A minute aggregation of pale reddish-brown crystalline felspar, quartz, and common schorl. A². Same with felspar, somewhat darker, and more crystalline. A³. Same mixture, with lighter-coloured felspar, and the schorl distributed in small contemporaneous veins, as well as intermixed in the mass. B. Felspar same as A nodules of quartz, and minute portions, apparently crystalline, of *chlorit*? Structure semiporphyritic. C¹. Minute aggregate of earthy felspar, of a pale dirty-red, quartz, and chlorit. C². Same with the felspar, less earthy, and of a deeper red. D. Porphyritic base, of a purplish-white, apparently a minute aggregation of earthy felspar and quartz, imbedded quartz in small nodules, and some crystalline felspar (semivitreous). D². The same with the quartz so much predominant in its base as to give it, at first sight, the aspect of a sandstone, or greywacke.* D³. Same with imbedded semivitreous felspar, and common felspar in various stages of decomposition (from the bed of the river Exe). D⁴. Base more felspathic, and of a deeper purple, much disintegrated, with the exception of the semivitreous felspar. Many cavities filled with earthy felspar. D⁵. Porphyry, base compact felspar, of a greyish-white, having imbedded small nodules of quartz, and penetrated by numerous cavities, apparently left by the disintegration of crystals of felspar, and the loss of the powdery matter so produced. (This remarkable character I have observed in more than one porphyry from Cornwall.) E. Base, a minute aggregation of earthy felspar, quartz, and chlorit, coloured green by the latter, imbedded minute crystals of flesh-coloured felspar, and small nodules of quartz. (Descent of Haldon on the eastern side.) All these occur in various stages of disintegration. Those porphyries approaching the nearest in colour to the marle which surrounds them are, when far advanced in this state, not readily distinguishable from that substance, the outline of the original fragment being so broken down and lost, that it appears to pass insensibly into the imbedding mass. Hence, perhaps, some geologists, of no inconsiderable authority, have been induced to suspect that these, and, by consequence,

* It is distinguishable, however, by its fusing readily before the blow-pipe into a vitreous globule. On breaking too the larger masses, the interior is found to be somewhat more felspathic.

the other fragments imbedded in the red marle were of a formation contemporaneous with itself.* A minute and accurate inspection of the coast between Dawlish and Teignmouth must, however, I think, convince us of the truth of the commoner, or at least earlier, opinion which regards them as derived from the breaking up of the inferior strata. Other fragments imbedded in the marle are *greywacke, or compact sandstone*. F¹. *Compact greywacke*, of a dirty-white, much ironshot, having the aspect of a sandstone with a very small intermixture of argillaceous matter. F². *Compact greywacke*, quartz more predominant, and closely aggregated ironshot throughout of a reddish-grey. F³. *Same*, of a greyish-black, with contemporaneous veins of white quartz. G¹. *Black compact siliceous rock*, of a very close texture, resembling *lydian stone*. G². *The same* intersected in all directions by small veins of ragged quartz, so as nearly to resemble a breccia, occasional cavities filled with brown manganese ochre. H. Small fragments, apparently of the reddish *greywacke slate*, provincially termed *shillat*. This list might be increased by the enumeration of some more trifling varieties in the compact greywacke. *Calcareous rock*. I¹. *Semicrystalline limestone*, of a dirty-white, without organic remains. (Near the headland termed the parson and clerk.) I². *Dark grey limestone*, without organic remains, near Teignmouth. I³. *Same*, full of coralloids. Same spot.

It may be added, that insulated fragments, and occasionally crystals of semivitreous felspar are met with in the marly beds. A pit near Exeter afforded me an interesting specimen of three macles united in a single groupe. Generally this variety of felspar seems to have resisted the action of those causes which have produced the disintegration of its parent rock.

The fragments which I have attempted to describe are, for the most part, mixed promiscuously in the same strata. Occasionally particular substances predominate, but rarely, or never (as far as my observation went) to the total exclusion of all others. The porphyritic and quartzose fragments have usually their angles but slightly rounded; in some cases, not even perceptibly so. The calcareous portions have generally more the appearance of being worn (as would from their softness be the case) by attrition. These facts, added to the consideration that the porphyritic or felspathic portions bear no resemblance to the only solid rock which appears to be subordinate to this formation (namely, the amygdaloid of Thorverton) will, perhaps, be sufficient to establish the mechanical origin of the breccia in question. We shall then have to inquire whence its contents might be derived.

The *calcareous fragments* bear a resemblance sufficiently close to the limestones of Chudleigh and Babicomb. A limestone

* See Dr. Kidd's Essay, p. 109. I have every reason to believe that in this opinion my respected friend was by no means singular.

more abundant in coralloids is found yet nearer in the neighbourhood of Lindridge. The limestone also of Bickington, near Ashburton, contains many of these fossils. The fragments of the *greywacke* class may readily be traced to the rocks of that species which lie in most places immediately beneath the marl, and with which indeed the transition limestones of the country are interstratified. Of the *granitic and porphyritic* fragments, those marked A, 1, 2, 3, have all the characters of a rock frequently occurring on the confines of the Dartmoor granite, and not unfrequently intermixed either as veins or irregular masses, both with that rock and with the neighbouring schistus. It will be found thus distributed a little beyond Bovey Tracey. I have met with aggregates nearly similar at the junction of granite and schist at Ivy Bridge, and at Buckland in the Moor. The remaining felspathic fragments I have little hesitation in referring to that class of rocks which are known by the name of *Elvans*, and found in numberless instances traversing the metaliferous slate of Devon and Cornwall. In the latter county, they have been more frequently observed, both from the greater extent of those sections of the Killas which are offered by its coasts, and the frequency and magnitude of the excavations made by the miner. In Devon I have noticed them near Tavistock, near Buckland Monachorum, and in the course of the West Okement, and have no doubt that they might be detected in various other quarters, especially near the junctions of the granite and slate. The only instance of dissimilarity which I have observed is the occurrence in some cases of large crystalline masses of the felspar, which I have termed *semivitreous*, and stated to form a part of the rocks marked D 1, 2, 3, 4. My limited collection of *Elvans* does not afford any analogous specimen, but when we remember that nearly every mine in Cornwall presents one or more varieties of this rock, and how endless are the minute shades of difference which characterize them, it will, I think, be allowed that there is nothing improbable in the supposition that the whole contents of this breccia have been furnished by the inferior rocks of its immediate neighbourhood, by those, perhaps, whose edges are yet covered by it at a depth to which our labours and investigations have but little chance of penetrating.

You will scarcely need to be reminded that Mr. Leonard Horner arrived at a like conclusion from his examination of the rock marl and adjacent strata in Somersetshire.* It struck me as singular that among the fragments which fell under my inspection I observed no traces of hornblende rock, or greenstone, although the latter especially, and in some instances small portions of the former are to be found on the borders of Dartmoor. The cliffs of Henoch present so large and striking

* See Geological Transactions, vol. iii.

a specimen of greenstone as long ago to have attracted notice, and the town of Bovey Tracey stands on a rock of the same nature.

I forbear to speculate on the probability that the whole extent of the red marble was produced by the degradation of the rocks which have left their fragments still imbedded in its mass. The total absence of those organic remains, which occur so abundantly in the strata immediately below as well as above, and the general want of consolidation in its various and heterogeneous beds, certainly argues that its formation took place under different circumstances, and by a different process from that of the subjacent slate and limestone, or the superincumbent lias. The strata at Dawlish are not everywhere of uniform thickness; they dip at an angle hardly exceeding 15 to S.E. by S. On this coast they are usually capped by the debris of the green sand formation which covers the neighbouring heights of Haldon. At Dawlish these debris are much more plentiful than at Teignmouth. It may be remarked, that while they cover so large a space towards the coast, they are of much scarcer occurrence on the plain of Bovey, which lies under the opposite declivity of Haldon. Some, however, apparently water worn, are found on that tract. I cannot conclude without expressing a wish that the whole extent of this formation were carefully examined by some abler and more instructed observer.

ARTICLE IV.

A new Method of constructing geometrically the Cases of Spherical Triangles by a Development of their Parts in Plane. By Mr. W. L. Birkbeck. (With a Plate.)

(To the Editor of the *Annals of Philosophy*.)

SIR,

London, March 10.

THE advantage of constructions as means of checking the calculations of plane trigonometry has been generally so apparent, that many trigonometrical writers have given simple geometrical constructions for the cases of plane triangles. With regard, however, to spherical trigonometry, a different course has been adopted; and as the consideration of the position of the various circles of the sphere first led to the problem of finding certain parts of the triangles formed by their intersections from others that might be given, the writers on this subject have thought it necessary that all these circles should be represented on a plane, as they actually appear on the sphere. Of the several methods of doing this, the stereographic projection has

generally claimed the preference; but although this has the advantage of exhibiting the position of any spherical triangle in relation to the several circles of the terrestrial or celestial sphere, yet the length of the constructions required to represent truly only *one* triangle renders it by no means the most desirable way of arriving at those parts by geometrical construction. The method pursued in this short essay considers the triangle as the spherical surface of a tetrahedron whose vertex is the centre of the sphere, and by imagining the sides of this tetrahedron laid down on a plane, obtains the required parts of the triangle by constructions that may be performed with facility by the help of a scale of chords. The figures here used are contained within much narrower limits than in most instances of stereographic projection, since the centres of some of the circles in it often fall at a very great distance. The constructions in the present paper may, it is presumed, be called new; for although M. Mauduit has constructed the first case, and part of the second, in a manner somewhat similar, yet his method is considerably longer; and the remaining part of the second, and the whole of the third case, I believe, have not been constructed except by projection, by any trigonometrical writer. As by means of the supplemental triangle the six cases of trigonometry are reducible to three, it has only been thought necessary to give the detail of three cases in this paper.

Case 1.—Having given the three sides AB , BC , AC , of a spherical triangle (Pl. VI.) fig. 1, to find any one of the angles as A .

With the chord of 60° as radius, taken from any convenient scale of chords, describe a circle $C'BA C$, fig. 2, and by means of the same scale set off the arcs AB , BC , AC , equal to the three given sides respectively; from the centre O draw OC' , OB , OA , OC , and let fall the perpendiculars CD , $C'E$, to OA , OB , meeting in P ; from P apply PN equal to CD , meeting OA (produced if necessary) in N , then the angle NPD is equal to the angle A , required; the number of degrees, &c. in which may be ascertained either by a scale of chords or a protractor.

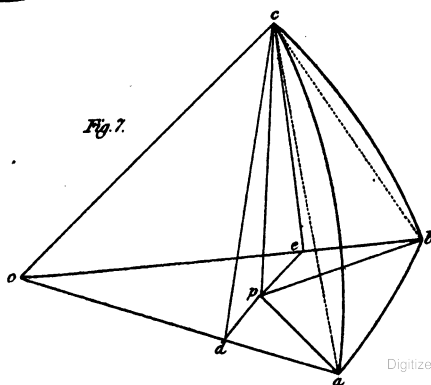
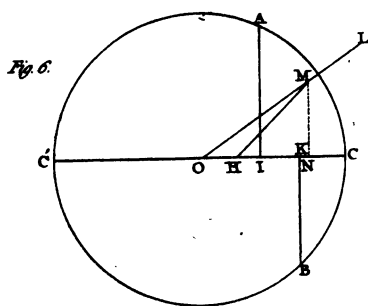
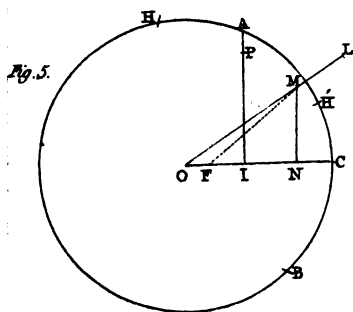
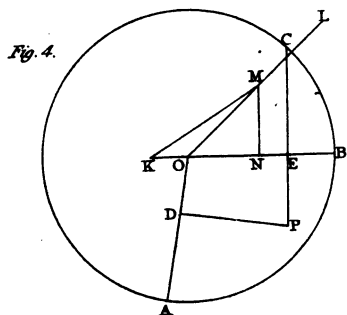
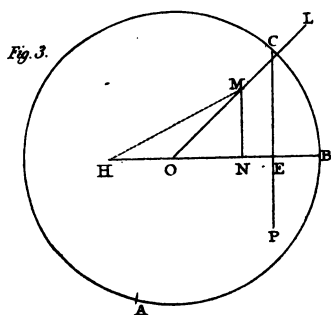
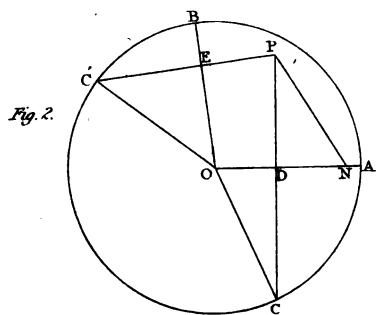
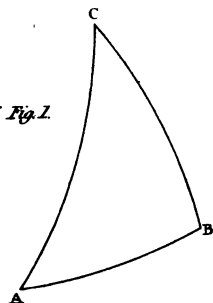
Case 2.—When two sides AB , BC , and the included angle B , are given.

First, to find the third side AC .

From any point O , as a centre, fig. 3, with the chord of 60° , describe a circle as before, set off the arcs BA , BC , equal to the given sides, and make $BO L$, equal to the given angle; draw CE perpendicular to OB , and on OL take $OM = CE$, and make MN parallel to it; on CE produced take $EP = ON$, and set off $NH = PA$. The distance MH is equal to the chord of the required side to the radius OB .

Secondly, to find either of the other angles, as the angle A .

With the chord of 60° describe a circle as before, and set off



the arcs $B A$, $B C$, fig. 4, equal to the given sides, and make $B O L$ equal to the given angle; let fall $C E$ perpendicular to $O B$; on $O L$ take $O M = C E$, draw $M N$ parallel to it, and on $C E$ produced take $E P = O N$. Join $O A$, let fall $P D$ perpendicular to it; take $N K = P D$, and join $M K$; then $M K N$ is equal to the required angle A .

Case 3.—When two sides $B C$, $C A$, and an angle A opposite one of them are given.

First, to find the third side $A B$.

With the chord of 60° as radius, and from any centre O , fig. 5, describe a circle, set off the arcs $C B$, $C A$, equal to the given sides, and make $C O L$ equal to the given angle, draw $A I$ perpendicular to $O C$, take $O M = A I$, and draw $M N$ perpendicular to $O C$; from M apply $M F =$ chord $B C$, meeting $C O$ produced, if necessary, in F ; make $I P = O N$, and from P as a centre, with the distance $F N$, intersect the circumference of the circle in H and H' : the arc $C H$, or $C H'$, is equal to the third side required.

Secondly, to find the angle B opposite the other given side.

With the chord of 60° describe a circle as before, take the arcs $C B$, $C A$, fig. 6, equal to the given sides, and make $C O L$ equal to the given angle; draw $A I$, $B K$, perpendicular to $O C$, take $O M = A I$, and from M apply $M H = B K$, meeting $C O$, produced, if necessary, in H : the angle $M H C$, or its supplement $M H C'$ is equal to the required angle B .

Thirdly, to find the angle C included by the given sides.

Find the third side $A B$ by the first part of this case, and the angle C from the three sides by Case 1.

This third Case it is well known frequently admits of two solutions, and in those instances in which the data lead to a double solution, these constructions will accordingly give two values for each of the required parts. In the first part of the Case, if H' either falls upon C , or on the same side of C with B , there will be only one value of the side $A B$, viz. $C H$. In the second part of the case, if either of the angles $M H C$, $M H C'$, taken as the value of B make $\frac{1}{2}(A + B)$ of the same affection with $\frac{1}{2}(B C + C A)$ then either of these angles may be that required, but if only one of the values of B is in accordance with the theorem, $\frac{1}{2}(A + B)$ like $\frac{1}{2}(B C + C A)$, this value is that required.

In the third part of the Case, the angle C will have one or two values according as the side $A B$ has one or two values.

Without giving the demonstration of these constructions with all the rigour of the Euclidean geometry, the mathematical reader will, it is presumed, be convinced of their accuracy from the following general explanation. Suppose $o c b a$, fig. 7, to be a spherical tetraedron whose radius, $o a$, is equal to the radius of the scale of chords made use of in the preceding constructions, and whose convex surface, $a c b$, forms the triangle proposed to

be solved. Imagine cp to be drawn perpendicular to the plane oba ; let cd, ce , in the planes oca, acb , be perpendicular to ca, ob , respectively; then if pd, pe, pa, pb, ca, cb , be joined, it is obvious that the angles $cpd, cpe, cpa, cpb, odp, oep$, are all right angles, that the angle, cdp , measures the inclination of the planes, oca, oba , and is consequently equal to the spherical angle at a , and that in like manner cep is equal to the spherical angle at b : moreover ca, cb , are the chords of the sides ca, cb . This premised, it will appear that in Case 1, CD, CE , are equal respectively to cd, ce , and that P is situated in the sector BOA , as p is in boa ; so that $PD = pd$, but $PN = CD = cd$, and the right angle $PDN = cpd$; therefore, the angle $NPd = cdp =$ spherical angle a .

Case 2.—Here CE , fig. 3, $= ce$, or $OM = ce$, and since $NOm =$ given angle $b, = cep$, and angle $MNO =$ a right angle; therefore, the triangle OMN is equal to cep , and $MN = cp$: also because $OE = oe$, and $EP = ON = ep$; therefore, P is situated with respect to the points B, A , as p is with respect to b, a ; consequently $PA = pa$, but $NH = PA$, and $MN = cp$, wherefore the right angled triangle, MNH , is equal to cpa , and $MH = ca =$ chord of side ca .

In the second part of this case, P is found as before, corresponding to p , and, therefore, PD , perpendicular to OA , is obviously equal to pd , but $NK = PD$, or pd , and $MN = cp$, wherefore the right angled triangle MNK is equal to cpd , and the angle MKN equal to cdp , or to the spherical angle a .

Case 3.—Here it is obvious that AI , or OM , is equal to cd , and the angle MON is equal to the given spherical angle a , that is, to cdp ; therefore, the triangle MON is equal to cdp ; hence $MN = cp$, and $ON = dp$. Also since $MF =$ chord $BC = bc$, the triangle FMN is equal to bcp , and, consequently, $FN = pb$; moreover, because $IP = ON = dp$, therefore P is situated with respect to the points O, C , and arc CA , as p is with respect to o, a , and arc ab , but pb has been found $= FN$, therefore, the intersection of an arc described from the centre P , with the radius FN , will determine the point H corresponding to b , and the arc CH will be equal to ab .

In the second part of Case 3, AI , or OM , is equal to cd , and BK , or MH , to ce . Hence the angle MON being equal to the spherical angle a , that is to cdp , the triangle MNO is equal to cpd , and, therefore, $MN = cp$; but $MH = BK = ce$, therefore, the triangle MNH is equal to the triangle cep , and the angle MHN , or MHC , equal to cep , or to the spherical angle b .

I am respectfully yours,

W. L. BARKNECK.

ARTICLE V.

On Atropium, a new Alkaline Body in the Leaves of Atropa Belladonna. By M. Rudolph Brandes.*

M. BRANDES first states, with reference to another paper of his which has not come to hand, that he found, in an analysis of the leaves of atropa belladonna, that pure alkalies precipitated a substance from the decoction of them which had alkaline properties like morphia. He then boiled a filtered decoction of the leaves of atropa belladonna with pure magnesia, washed the powder thus obtained with water, and boiled it in alcohol. This hot solution in alcohol had likewise alkaline properties, and proves, therefore, that they were not to be ascribed to the alkali employed in the first experiment. He then goes on to state the following experiments :

I boiled two pounds of dried leaves of atropa belladonna in a sufficient quantity of water, pressed the decoction out, and boiled the remaining leaves again in water. The decoctions were mixed, and I added some sulphuric acid in order to throw down the albumen and similar bodies ; the solution is thus rendered thinner, and passes more readily through the filter. The decoction was then supersaturated with potash, by which I obtained a precipitate that, when washed with pure water and dried, weighed 89 grs. It consisted of small crystals, from which, by repeated solution in acids, and precipitation by alkalies, the new alkaline substance, atropium, was obtained in a state of purity.

Properties of Atropium.—The external appearance of atropium varies considerably according to the different methods by which it is obtained. When precipitated from the decoction of the herb by a solution of potash, it appears in the form of very small short crystals, constituting a sandy powder. When thrown down by ammonia from an aqueous solution of its salts, it appears in flakes like wax, if the solution is much diluted ; if concentrated, it is gelatinous like precipitated alumina ; when obtained by cooling of a hot solution in alcohol, it crystallizes in long, acicular, transparent, brilliant crystals, often exceeding one inch in length, which are sometimes feathery ; at other times starlike in appearance, and sometimes they are single crystals. Atropium, however, is only obtained in such a crystalline state when rendered perfectly pure by repeated solution in muriatic acid, and precipitation by ammonia : a solution of impure atropium which I made by boiling the decoction of the leaves of atropa belladonna with magnesia, and dissolving the precipitated alkali in hot alcohol, has not, during half a year, deposited any crystals but merely flakes.

* From Schweigger's Journal, vol xxviii. p. 1.

The colour of atropium depends in a great measure upon its state of purity. When perfectly pure it is snow white, otherwise it is more or less yellowish-white. When pure, it has no taste. When strongly heated, it is decomposed, is charred, and gives out an empyreumatic smell.

Cold water has hardly any effect upon dried atropium, but it dissolves a small quantity when it is recently precipitated; and boiling water dissolves still more.

Cold alcohol dissolves but a minute portion of atropium, but when boiling it readily dissolves it. Atropium, however, is much more difficultly dissolved by boiling alcohol than morphium, and while the solution is cooling, the greatest part is again deposited. In order to ascertain whether, after the crystallization of the atropium, the remaining spirit contained any quantity of this body, I evaporated two drachms of it, but they left scarcely a trace of it.

Ether and oil of turpentine had even, when boiling, little effect upon atropium. Hot oil of almonds dissolved a much greater quantity of it, and seemed to become less fluid. The experiment, however, was made on so small a scale that I was unable to ascertain whether the oil had undergone the same alterations which Chevreul and Braconnot observed in saponified fat, and which Boullay believes he has produced by picrotoxiuim.

Salts of Atropium.—Atropium forms salts with acids, the greater number of which are readily soluble in water; but I never have succeeded in obtaining them perfectly neutral, or in freeing them completely from adhering acid. Even when carefully washed and pressed between fine paper, they reddened litmus paper in most cases; but when care was taken, there was only a trace of free acid remaining, and which cannot be regarded as an objection to considering the atropium as the basis of salts; for the whole quantity of acid combined with the alkaline body would have produced a much more powerful effect upon litmus paper, had it not been attracted and neutralised by this substance.

Atropium and Sulphuric Acid.—Atropium, when slightly heated with concentrated sulphuric acid, is decomposed and blackens; a similar effect takes place when sulphate of atropium in its crystalline state is dried without being previously washed with alcohol, and deprived of the adhering acid. Diluted sulphuric acid dissolves atropium after some time, and more easily when the alkali has not been dried. Exposed to spontaneous evaporation, crystals of sulphate of atropium appear, of which I, however, could not ascertain the form with sufficient accuracy, there existing a great variety of crystals, and the planes being very imperfect. The most perfect seemed to me to be rhomboedrical tables and prisms with square bases; the lateral planes partly truncated, and the lower acute angle of the new planes

often reaching down to the middle of the prism. These triangular planes appear frequently on the crystals of sulphate of atropium, and are distinguishable by their great lustre.

The sulphate of atropium is soluble in four to five parts of cold water; it seems likewise to be soluble in alcohol; for when I had poured the remaining liquid from the crystals, and begun to wash them with spirit of 90 per cent. alcohol, a quantity of the salt seemed to disappear, and this cold solution in alcohol, when ammonia was added, became milky by the precipitation of atropium.

It does not attract moisture from the air, when as much as possible freed from its adhering sulphuric acid, but, on the contrary, it loses its water of crystallization. It is decomposed when strongly heated, and becomes black.

The salts of barytes deprive this salt of its sulphuric acid, and the carbonated as well as the pure alkalies precipitate atropium from the solution.

Composition of Sulphate of Atropium.—(A.) Three grains were exposed on a watch glass to a moderate heat. While the water of crystallization was evaporating, it became slowly opaque, assumed a milk-white colour, on a few places it had begun to blacken, and resembled enamel in appearance. When it had ceased to lose weight by heat, the loss amounted very nearly to three-fourths of a grain, or 25 per cent. which is to be considered as water of crystallization. The salt was only in a very slight degree decomposed, and was almost entirely soluble in water.

(B.) Three grains of the same salt were dissolved in water, and ammonia was added to the solution. The precipitate, when collected on a filter, weighing $3\frac{1}{4}$ grs. washed and dried amounted to a little more than $1\frac{1}{4}$ grs.

(C.) The filtered liquid from (B.) after being supersaturated by acetic acid was mixed with a solution of muriate of barytes. There was as much sulphate of barytes obtained after heating it red, as would indicate almost 36 per cent. of sulphuric acid.

The sulphate of atropium, therefore, is composed of

Atropium	39
Sulphuric acid	36
Water	25
	<hr/>
	100

I must confess I had some doubts as to the correctness of this analysis, for on comparing it with that of the sulphate of strychnium by Pelletier and Caventou, and that of sulphate of picrotoxium by Boullay, and my own analysis of sulphate of morphia, it appears that all these three alkaline substances are very low in saturating power; while the atropium would, according to my analysis, neutralise a much larger quantity of sulphuric

acid. If, however, there was only a slight inaccuracy in the analysis, or if there had remained some free sulphuric acid in the salt, this of course must make a great difference in the result, the experiments being made with so small a quantity. I therefore, thought it necessary to repeat my analysis with a larger quantity of salt, for which purpose I dissolved a new quantity of atropium in diluted sulphuric acid, and exposed the solution to spontaneous evaporation. I obtained beautiful crystals, which I washed with alcohol, and repeatedly pressed them between fine blotting paper. Thus I obtained 18 grs. of sulphate of atropium.

Five grains of this salt were carefully heated to expel the water. The loss amounted to $1\frac{1}{4}$ gr. which indicates 25 per cent. water of crystallization.

Ten grains were dissolved in water, and decomposed by muriate of barytes. The weight of the sulphate of barytes was 11 grs. equal to 36.13 per cent. of sulphuric acid.

After the deduction of 25 water and 36.13 sulphuric acid, there remains 38.87 for the atropium, and the composition of sulphate of atropium will be :

Atropium	38.87
Sulphuric acid	36.13
Water	25.00
	<hr/>
	100.00

The close approximation in the results of both analyses must evidently lead me to the conclusion that the error could not be very great. My doubt as to the great saturating capacity of the atropium induced me, however, to make a third experiment. I took atropium, obtained by cooling a hot spirituous solution, put it into some fresh alcohol, and added sulphuric acid until it was dissolved. The spirituous solution was completely clear, and yielded, after spontaneous evaporation, beautiful crystals of sulphate of atropium. After washing them with ether until this fluid ceased to redden litmus paper, I dried the salt between fine blotting paper.

With this salt I repeated my analysis. By drying three grains I found the water of crystallization amounting to nearly 24 per cent. Three grains were dissolved in water, and the solution reddened litmus paper slightly, notwithstanding repeated washing with ether. The sulphuric acid was precipitated by a solution of muriate of barytes. The weight of the sulphate of barytes, after exposing it to a red heat, was 3.25 grs. indicating 1.113 gr. of sulphuric acid, equal to 37.1 per cent.

The composition of sulphate of atropium, according to this analysis, is, therefore,

Atropium	38·9
Sulphuric acid.	37·1
Water	24·0
	<hr/>
	100·0

This I thought sufficient to remove all doubts as to the great saturating power of atropium. It proves the peculiar nature of this alkaline body, when compared with the rest of the seanalogue substances, all of which are capable of neutralising only a very small quantity of acid. Even picrotoxium, though according to Boullay, only crystallizable when an excess of acid is present, as appears to be the case with atropium, is capable of combining with only nine per cent. of sulphuric acid. I have little doubt that the mean number of these three analyses will approach very near to truth, and that, therefore, sulphate of atropium is composed of

Atropium	38·93
Sulphuric acid	36·52
Water	24·55
	<hr/>
	100·00

It seems as if atropium is capable of combining in different proportions with sulphuric acid. I once added an excess of sulphuric acid to atropium diffused in water, and I observed a sudden formation of long prismatic crystals, which seemed to be bisulphate of atropium. These crystals required several hundred times their weight of water for solution, while the salt of atropium which I analysed was soluble in four or five parts. It is well known that with picrotoxium the case is similar. I made several experiments to prepare that salt, but I could succeed only when I diffused atropium in alcohol, and then added an abundant quantity of sulphuric acid. By this process the salt quickly appeared as a crystalline precipitate.

Once as I evaporated a spirituous solution of sulphate of atropium, I was struck with the smell of bitter almonds. To determine whether the solution contained prussic acid, I diluted it, and added some solution of sulphate of deutoxide of iron: there did not, however, appear the least trace of a blue colour.

Atropium and Muriatic Acid.—The salt produced by the combination of muriatic acid and atropium appears in beautiful white brilliant crystals, which are either cubes or square plates, similar to the muriate of daturium. It is readily soluble in water and alcohol. By nitrate of silver the chloride is precipitated, and the alkalis precipitate pure atropium from it. Even after the most careful washing, I always observed some traces of free acid.

Analysis of the Muriate of Atropium.—I undertook this ana-

lysis not only to ascertain the composition of this salt, but also to obtain additional evidence as to its great saturating power.

(A.) Three grains of muriate of atropium, when dried at such a temperature that no decomposition of the alkali could take place, lost nearly $1\frac{1}{10}$ th gr.

(B.) Three grains of the same salt were dissolved in water, and the atropium thrown down by ammonia. After repeated washing with small quantities of water, it was collected on a filter, which, after being dried carefully, showed an increase of weight of nearly $1\frac{3}{10}$ th gr. which were atropium.

The composition of this salt is, therefore,

Atropium	39.19
Muriatic acid.	25.40
Water.	35.41
	<hr/>
	100.00

Atropium and Phosphoric Acid.—Atropium, when dissolved in a great quantity of phosphoric acid, and evaporated, forms a mass like gum, which attracts the moisture of atmospheric air, and deliquesces. If less phosphoric acid is used, the solution, after spontaneous evaporation, yields some quadrangular prisms, which salt is readily soluble in water, and is likewise readily dissolved by alcohol.

On other Salts of Atropium.—Nitric, acetic, and oxalic acid dissolve atropium, and form acicular salts, all soluble in water and alcohol. The acetate and nitrate of atropium are hygrometric. All are decomposed by the alkalies and by their carbonates.

Nitric acid acts much less powerfully on atropium than on strychnium, according to Pelletier and Caventou.

Observations on the Equivalents of Atropium.—I have shown that sulphate of atropium is composed of

Atropium.	38.93
Sulphuric acid	36.52
Water.	24.55
	<hr/>
	100.00

and that muriate of atropium consists of

Atropium.	39.19
Muriatic acid.	25.40
Water.	35.41
	<hr/>
	100.00

If we admit that in the alkalies from plants, the same laws exist as are ascertained with respect to those from inorganic bodies, we shall be better able to find their equivalent number, as I have shown in my analysis of sulphate of morphium. The

sulphate of atropium appears to contain three times as much oxygen in the acid as in the base; and as 36.52 sulphuric acid contain 21.865 oxygen, $\frac{21.865}{3} = 7.289$ will indicate the oxygen in 38.93 of atropium.

The muriates contain, according to Berzelius, in their acid, twice the oxygen which exists in the base, and 25.4 muriatic acid containing, according to Berzelius, 14.825 oxygen $\frac{14.825}{2} = 7.412$ will indicate the oxygen in 39.19 of atropium.

By comparing these two results, it will be found that they differ very little. I am, however, inclined to suppose the result of the analysis of the sulphate of atropium to be the most correct, and then 100 parts of atropium would contain 18.91 oxygen, a quantity far exceeding the oxygen found in any one of the similar alkalies.

There are, however, strong reasons for believing that the different salts which have been analyzed are bisulphates and bimuriates, and then the oxygen of the atropium would only amount to one half of the above-mentioned quantity. The salt which is above described as a bisulphate then would be a quadrisulphate. The oxygen in 38.93 atropium would then be 3.644, and 100 parts of atropium would contain 9.4 of oxygen.

The oxygen in the different parts of sulphate of atropium would be :

		Oxygen.
Atropium	38.93	3.644
Sulphuric acid	36.52	21.865
Water	24.55	21.654

The oxygen in the acid and in the water, therefore, are equal, and six times that of the atropium.

According to Berzelius's theory, the oxygen in the muriate of atropium would be :

		Oxygen.
Atropium	39.19	3.706
Muriatic acid	25.40	14.808
Water	35.41	31.784

The quantity of oxygen in the acid is four times as much as that in the atropium, and that of the water twice as much as that of the acid.

One hundred parts of dry sulphate of atropium contain

Atropium	51.59
Sulphuric acid	48.41
	<hr/>
	100.00

Or it consists of

Atropium	100·00	106·5
Sulphuric acid	93·83	100·0

One hundred parts of muriate of atropium would consist of

Atropium	60·68
Muriatic acid.	39·32
	<hr/>
	100·00

And

Atropium	100·0	154·3	would combine with
Muriatic acid.	64·8	100·0	

On the Alteration which Atropium undergoes when heated with Potash, and when burned.—When I tried some other substance obtained from the atropa belladonna by heating it with potash, I obtained a salt which, when supersaturated with acetic acid, produced with muriate of iron a red solution. I inferred from this, that there might be formed sulphochyazic acid. Suspecting this vegetable substance to contain atropium, I took some of this alkali as it had been precipitated from a decoction of atropa belladonna, and heated it with a solution of pure potash in a platina spoon. As soon as the action of the potash began, a distinct smell of ammonia was perceptible, and a glass rod with strong muriatic acid produced a white cloud when brought near to it. The residuum was dissolved in water, supersaturated with acetic acid, and tested with muriate of deutoxide of iron, which instantly produced a red colour.

This experiment did not always succeed. It proves, however, that under certain circumstances there may be formed a substance from atropium, which produces a red colour with deutoxide of iron, and which probably is sulphochyazic acid. Sulphur is, perhaps, a constituent of atropium. The potash I made use of had a slight trace of sulphuric acid.

I have been obliged to discontinue my experiments on the properties of this alkali. The violent headaches, pains in the back, and giddiness, with frequent nausea, which the vapour of atropium occasioned while I was working on it, had such a bad effect on my weak health that I entirely abstained from any further experiment.

I once tasted a small quantity of sulphate of atropium, the taste was not bitter but merely saline, but there soon followed violent headache, shaking in the limbs, alternate sensations of heat and cold, oppression of the chest, and difficulty in breathing, and diminished circulation of the blood. The violence of these symptoms ceased in half an hour.

Even the vapour of the different salts of atropium produces giddiness. When exposed for a long time to the vapours of a solution of nitrate, phosphate, or sulphate of atropium, the pupil

of the eye is dilated. This happened frequently to me, and when I tasted the salt of atropium, it occurred to such a degree that it remained so for 12 hours, and the different degrees of light had no influence.

This is sufficient to show the poisonous effect of atropium.

ARTICLE VI.

On some Capillary Metallic Tin. By James Smithson, Esq. FRS.
(To the Editor of the *Annals of Philosophy*.)

SIR,

Paris, Feb. 17, 1821.

M. AMPERE, a few days ago, accidentally in conversation, mentioned a fact to me which much excited my attention, as it appeared to me completely to confirm the explanation I had ventured to offer of the mode of formation of the capillary copper in the slag of the Hartz, printed in the *Annals of Philosophy* for July, 1820.

For some purpose of the arts, Mr. Clement formed a cylinder of copper, and, to give it strength, introduced into it a hollow cylinder, or tube, of cast iron. To complete the union of these two cylinders some melted tin was run between them. With the exact particulars of this construction, I am not acquainted, but the material circumstance is, that during the cooling of this heated mass, a portion of the melted tin was forced by the alteration of volume of the cylinders *through the substance* of the cast-iron cylinder, and issued over its internal surface in the state of *fibres*, which were curled and twisted in various directions. This form in the fibres of copper I had considered as very favourable to my hypothesis. Such was the tenuity of these fibres of tin that little tufts of them applied to the flame of a candle took fire, and burned like cotton.

This passage of melted tin through cast-iron has a perfect agreement with the passage of water by pressure through gold, and tends to elucidate and confirm the account of the celebrated Florentine experiment. Had the water on that occasion issued solid, it would have been in fibres.

This penetration of solid matters by fluids, by means of great mechanical force, will, perhaps, come to be thought deserving of more attention than has been yet paid to it; besides any scientific results to which the consideration of it may lead, it may be found to afford compound substances, not otherwise obtainable, and of value to the arts.

I am, Sir, your most obedient servant,

JAMES SMITHSON.

ARTICLE VII.

Results of a Meteorological Register kept at Lancaster in the Year 1820. By Mr. J. Heaton.

1820.	THERMOMETER.				BAROMETER.			WEATHER.		WINDS.							
	Highest.	Lowest.	Mean.	Mean diurnal variation.	Highest.	Lowest.	Mean.	Wet days.	Fair days.	S.	S.W.	W.	N.W.	N.	N.E.	E.	S.E.
Jan.	26 48°	3 16	32° 8'	7° 9'	9 30° 85	19 28° 68	29° 83	16	15	11	1	0	0	3	10	3	3
Feb.	7 50	18 25	38° 9	8° 2	27 30° 31	23 29° 01	29° 92	10	19	11	4	1	0	2	2	6	3
March. ...	29 54	6 23	41° 0	10° 7	18 30° 45	23 28° 93	29° 86	12	19	2	9	6	1	5	4	0	4
April.	23 67	7 37	48° 8	11° 5	24 30° 65	8 29° 26	29° 92	9	21	22	11	7	3	2	2	2	1
May.	23 75	5 33	53° 5	13° 1	1 30° 34	30 29° 13	29° 73	17	14	6	17	5	0	0	0	1	2
June.	27 77	11 45	58° 3	14° 7	27 30° 31	1 29° 33	29° 92	12	18	1	9	11	6	1	1	1	0
July.	31 76	1 46	62° 0	13° 4	1 30° 27	18 29° 35	29° 94	11	20	2	2	13	1	1	1	6	3
August. ...	3 74	23 41	59° 8	10° 7	11 30° 24	28 29° 44	29° 81	20	11	1	19	5	0	1	1	4	0
Sept.	12 72	19 41	56° 1	11° 2	8 30° 31	21 29° 32	29° 92	15	15	5	4	10	3	2	1	2	3
October. .	8 60	12 36	47° 0	9° 4	4 30° 60	17 28° 55	29° 59	17	14	6	1	5	6	0	0	8	5
Nov.	8 55	16 30	43° 0	7° 9	29 30° 35	1 29° 40	29° 85	11	19	8	4	1	0	1	6	7	3
Dec.	10 52	16 29	41° 2	5° 7	19 30° 29	16 29° 51	29° 94	15	16	6	7	2	0	0	4	8	4
June 77		Jan. 16	48° 53	10° 37	Jan. 30° 85	Oct. 28° 55	29° 854	165	201	61	88	66	20	18	32	48	33

ARTICLE VIII.

A Mathematical Inquiry into the Causes, Laws, and principal Phenomena of Heat, Gases, Gravitation, &c. By John Herapath, Esq.

DEAR SIR,

Cranford, near Hounslow, March 21, 1821.

THE following mathematical investigation of the causes, laws, and principal phenomena, of heat, gases, gravitation, &c. was drawn up about 10 months since, for the purpose of being laid before the public, in the Transactions of the Royal Society. A knowledge that the Royal Society had been desirous of having these subjects investigated, and a belief that they would, therefore, have been pleased with this mark of my respect, and have caused the subject to be sifted to the bottom, were the motives which induced me to present them with the first fruits of my labours. Having, however, notwithstanding the marked kindness of the President, and another highly distinguished and enlightened member of that Society, experienced from others an unlooked-for, and, I might almost venture to say, an illiberal opposition for upwards of nine months, I have thought it expedient to withdraw this paper, together with another which I composed at the suggestion of Sir H. Davy, confirming by experiment my views on the more material points, in order to bring them both before the public through a different channel.

It is not my intention here to detail the discouraging and extraordinary line of conduct which has been pursued in this affair, because I shall have it in my power, at the end of these papers, to enter fully into particulars; but I request you will have the goodness to print the communications just as they now stand; for the circumstances of the case being such that it will be necessary publicly to invite the attention of the whole Royal Society to this subject, I wish to make no alterations, but to give those who have opposed me a fair opportunity of overturning that against which they have not, in so long a period, produced one valid objection; or of justifying a conduct, which, though but belonging to a part, might, possibly, in the eyes of the world, from the importance of the subject, produce serious reflections on the whole of a Society, so long distinguished for its candour, liberality, and prompt encouragement, of scientific pursuits.

I have the honour to be, dear Sir,

Your most obedient servant,

To Richard Phillips, Esq.

JOHN HERAPATH.

On the Physical Constitution of the Universe. (In a Letter to Davies Gilbert, Esq. MP: FRS. &c.)

DEAR SIR,

In the following memoir, which I have to request you will do me the honour of submitting to the consideration of the Royal Society, I have endeavoured to unravel the causes of some of the leading phenomena of the universe, such as those of heat, gravitation, &c. That a correct judgment may the more easily be formed of the nature of this memoir, I have prefixed a brief account of the train of thought I pursued in my investigations, which will tend to throw a considerable light over what follows. And because I conceive no theory established, however well it may accord with phenomena, unless it can be shown all others do not, I intended to take a review of some of the principal hypotheses that have been advanced and supported by different philosophers, and to show wherein I think them defective; but afterwards considering that this would extend the memoir to a much greater length than I wish, and that I might probably be obliged to press a little hard on the favourite opinions of some respectable philosophers, I have for the present laid this design aside. For similar reasons I have omitted all comment and comparisons, except what are necessary for the elucidation of the subject. I have likewise, in consequence of a conversation that I lately had with W. Clayfield, Esq. materially changed the plan of this memoir, so as to render the connexion and dependence between the principles and consequences more apparent. And in order to trespass as little as possible on the time of the Royal Society, I have only demonstrated some of the elementary propositions; reserving the proofs of others to a future period, when, if the little that I have done receive the countenance of the Royal Society, I might be induced to take a more elevated view of the subject, and to touch upon other things that are not mentioned.

The Royal Society will easily perceive that I am indebted for the hint of the cause of gravitation to Sir Isaac Newton, though, perhaps, it will be found, that I have carried the idea much further, and have extended it to the development of a much greater variety of phenomena than he could have anticipated. It would be preposterous for me in this place to name other distinguished philosophers to whose accurate experiments and luminous views I have, in the course of my inquiries, been under great obligations; but I cannot let slip this opportunity of acknowledging, that if it shall appear I have been any way successful in my solitary rambles through these exalted regions, it is probably in a great measure owing to your kind encouragement and directions, and to the flattering approbation you were pleased to bestow on my juvenile efforts so long ago as the beginning of 1809, when I had the pleasure of being introduced to you by my late respected

friend W. Perry, Esq. of Winterbourne. Without such stimuli, I might never have had sufficient confidence in myself to tread those intricate and almost trackless paths of science.

An Analytical Inquiry into the Cause of Gravitation, Heat, &c.

Several years ago, namely, in July 1811, while amusing myself with calculating some of the lunar equations from theory, I was induced to try to compute the annual equation to the moon's mean motion, somewhat after the manner in which Newton has calculated the magnitude of the variation. The result of this calculation, which considerably exceeded the quantity given in the tables of Halley, the only ones I then had, very much surprised me. At first I thought I had committed some error, or made some erroneous assumption; but on re-examining the calculus, and making every allowance which I thought might have any influence, I satisfied myself, that as far as my fundamental principles were correct, nothing was neglected which could affect the result to any thing like the magnitude of the difference. At another time it occurred to me, that the quantity of the equation, as given in the tables, might possibly be itself too small. I, therefore, set myself about correcting it from the observations at the end of Halley's tables; but so far from solving the difficulty by this means, I found the difference much greater; for, as far as I remember, the maximum of the equation, I found to be, instead of $11' 49''$, only about $11' 17''$; that is, but a few seconds greater than the quantity given in the very correct tables of Burg. Baffled, therefore, in this attempt to reconcile observation and theory, I conceived that the quantity determined from observation must be the result of two opposite equations, one of which had escaped the cognizance of theory. And in this opinion I seemed to be more confirmed by another calculation of this equation, by means of an exponential theorem I had just before discovered, and by observing that the computation of the same equation by Machin, on the principle of an equant, came out also much greater than the quantity by observation. It is true I was for a little while staggered in my opinion by the statement of Newton, in the scholium to prop. 35, book 3, of the Principia, in which he says, that he had calculated the mean greatest quantity of this equation from the theory of gravitation at $11' 49''$. But as I observed that he simply named the result without even hinting at the method of calculation, though just beneath, in the same scholium, he minutely enough describes his calculus of one or two other equations of considerably less difficulty; and as I had observed that the quantity I had brought out would coincide with his, if diminished in the ratio of the moon's synodical to her sidereal period, I thought it very probable that Newton had pursued the same course that I had; and that finding his numbers would agree with observation, if diminished in the said ratio of the synodical to the sidereal period;

he diminished them, thinking there might be a reason for it which he did not perceive; but not being thoroughly convinced, he chose rather to omit than describe a calculation, with every part of which he was not perfectly satisfied. This at least appeared to me a very plausible way of accounting for Newton's silence; but whether it be a correct one, it is, perhaps, not worth the trouble of discussing, especially since the complete calculation of this equation, by the celebrated Laplace, in the *Mécanique Céleste*, shows, that whatever may have been Newton's method, it was, as well as my own, much too loose and inaccurate to be depended on. It is, however, remarkable, that all, or most, of the calculations of the ann. equa. hitherto made from theory, give the quantity of this equation greater than observation; and this is even the case with Laplace's.

Having now, as I thought, satisfactorily accounted for the difference between Newton's numbers and mine, I became more strongly persuaded in myself that the tabular magnitude of the ann. equa. was the difference of two equations; and, therefore, I frequently tried to unravel the cause and magnitude of the indeterminate one, but without success. At length, about the middle of the following September (1811), my attention being involuntarily turned to a consideration of Newton's opinion respecting the cause of gravitation, I fancied that I saw a true solution of the difficulty in question, as well as a complete development of the cause of gravitation. If, argued I with myself, gravitation depends upon the action of an elastic medium, such as Newton supposes, which grows rarer and rarer as you approach the dense bodies of the sun and planets, there ought to be some reason for this variation of density; and as Newton has not, as far as I could perceive, given any, I began to consider what it might * be. And after some little thinking, it occurred to me, that if this medium be of the same nature as our atmosphere and other gaseous bodies; that is, if it be capable of being expanded by heat, and contracted by cold, then, the sun being a very hot body, and the heat being so much the greater the nearer we are to him, the density of the medium ought, therefore, to decrease with a decreasing, and increase with an increasing distance, the same as Newton would have it. And because we find by experience that dense solid bodies receive heat more strongly than much rarer ones, particularly than gases, the dense bodies of the planets being heated by the solar rays as well as by the medium about them, ought, it appeared to me, to be hotter than this medium, and consequently ought to produce the same effects on the medium as the sun, though not in so great a degree. Therefore if, as Newton imagines, the

* The only accounts I had seen of Newton's ideas of this subject were in his *Optics*, and at the end of the *Principia*. I have, however, lately read a letter that he wrote to Mr. Boyle, printed in Bishop Horsley's edition of his works, wherein he gives his opinion of this fluid medium as being of the same nature as our air.

particles of the planets be impelled towards the sun by the inequality of pressure on their further and nearer sides, the denser parts of the medium pressing more forcibly than the rarer, the same reason will likewise hold good why bodies should be impelled towards the planets and other material parts of the system.

And by considering these things further, it seemed to me that if such be the cause of gravitation, the intensity of the impelling force should be subject to the influence of two circumstances; namely, the number of particles in the central body, and their temperature; so that it becomes greater when either of these becomes greater, and less when it becomes less. But since the earth in its passage round the sun is sometimes at a greater and sometimes at a less distance from it; and since the regions which are nearer the sun are hotter than those which are more remote, the temperature, and consequently the attraction of the earth, should increase as the earth approached the sun, and diminish as it receded from it, so as to be greatest about the perihelion, and least about the aphelion. And this being the case, the moon must move in a contracted orbit, and swifter round the perihelion earth, and in a dilated orbit, and slower round the aphelion earth; by which means an equation to the moon's mean motion must be generated contrary to the ann. equa. and diminish it, the same as I had supposed some unknown equation ought to do with the theoretical annual equation to reduce it to the tabular.

Thus it happened that the inadequate method of computation I had adopted, brought out a quantity which so well accorded with my first theoretical views of the cause of gravitation, that I could not help placing great confidence in the theory I had embraced. I, therefore, carried on my speculations with that ardour which a strong prejudice in favour of the truth of my principles, and the sanguine hopes of succeeding in so great a problem as that of developing the cause of gravitation, might naturally be supposed to inspire; but I soon found that before I could proceed any further, I must establish the cause of heat, and reduce its phænomena to mathematical laws. This I at first attempted to do by endeavouring to find out the relation which should exist between the masses of the particles of the ethereal medium and their repulsive force, in an equation connected with their distances from the sun. But being disappointed in this, and a great number of other attempts that I made, I became much dispirited, and was often on the point of forming a resolution never to consider the subject again. Indeed I frequently wished to persuade myself that the discovery was altogether beyond the reach of human ability; and with this view tried to thrust it entirely from my mind. Yet sometimes, when my thoughts were involuntarily turned this way, the idea that two

inanimate bodies could act on each other at a distance without some other means than that of a mere tendency, or inclination, in them to approach, would appear so strongly unphilosophical, and the apparent coincidence of several phenomena, with conclusions I had drawn from my notions of gravitation, so very seductive, that I could not avoid thinking the views I had taken were tolerably correct; and that there was only wanting the direction of some happy idea, which patient perseverance might possibly attain, to set the whole in a clear and irrefragable light. Thus between hope and despair, between unceasing attempts and mortifying failures, I continued until May 1814, at which time my ideas of heat underwent a complete revolution. Previous to this time I had conceived heat to be the effect of an elastic fluid; and on this supposition had repeatedly attempted to reduce its laws to mathematical calculation; but uniform disappointment at length induced me to give this hypothesis a careful investigation, by comparing it with general and particular phenomena. The result of this investigation convinced me that heat could not be the consequence of an elastic fluid. At the time I was making this comparison, I took every opportunity of examining how far the other hypothesis (which until now I had forgot was sanctioned by the names of Newton and Davy) agreed with phenomena, and was so well pleased with its simplicity, and the easy, natural manner in which the different phenomena seemed to flow from it, that I regretted having neglected it so long, and determined to consider it more attentively. A difficulty, however, soon appeared in the application of this theory of heat to gaseous bodies, which I had some trouble to conquer; for as I still adhered to the hypothesis of gases being composed of particles endued with the power of mutually repelling one another, I could by no means imagine how any intestine motion could augment or diminish this power. Here then I was involved in another dilemma; but after I had revolved the subject a few times in my mind, it struck me that if gases, instead of having their particles endued with repulsive forces, subject to so curious a limitation as Newton proposed, were made up of particles, or atoms, mutually impinging on one another, and the sides of the vessel containing them, such a constitution of aeriform bodies would not only be more simple than repulsive powers, but, as far as I could perceive, would be consistent with phenomena in other respects, and would admit of an easy application of the theory of heat by intestine motion. Such bodies I easily saw possessed several of the properties of gases; for instance, they would expand, and, if the particles be vastly small, contract almost indefinitely; their elastic force would increase by an increase of motion or temperature, and diminish by a diminution; they would conceive heat rapidly, and conduct it slowly; would generate heat by sudden com-

pression, and destroy it by sudden rarefaction; and any two, having ever so small a communication, would quickly and equally intermix.

Besides these, other properties equally consistent and gratifying, presented themselves; but as these were merely loose views of the subject, I soon resolved to examine it more rigorously, and to try if I could not bring it to the test of mathematical laws. In this, however, I met with a difficulty considerably superior to any I had yet encountered in the course of my analysis, and which, before I overcame it, gave me more real uneasiness than, perhaps, it can be imagined it should. But the truth is, my views of the subject expanded so much as I proceeded that even in this early stage I fancied I perceived, in the solution of the problem I was about, not only the discovery of the cause of gravitation, but also of the causes of all the other phænomena of nature; and my thoughts were, therefore, turned upon it with an intenseness and anxiety which I never before experienced, and which can scarcely be appreciated except by those who have been placed in a similar situation. To meet now, therefore, when I thought I had almost completed the discovery, with an obstacle which it baffled my utmost efforts to surmount, and which threatened destruction to the fabric I had so laboriously endeavoured to raise, was a shock I had hardly philosophy enough to withstand. However, as I had proceeded so far, and had been so much led away by the seducing coincidence of the consequences of my theory with phænomena, I determined to examine it thoroughly, and, if I should find it erroneous, to publish it together with the illustration of its errors, that if it could do no other good, it might serve for a beacon to prevent others from running against the rock on which my hopes and expectations had been wrecked.

The obstacle to which I allude is this: I saw directly I began to consider circumstances attentively, that if the constitution of things be such as I supposed, the ultimate atoms of all bodies, and, therefore, the particles of these gases, which I looked upon to be no more than these ultimate atoms, must be absolutely hard; they must admit of no breaking, splitting, shattering, or any impression whatever; and yet if the gases are to maintain their elastic property, and this property be the result of the particles mutually impinging on one another and the sides of the containing vessel, the particles, or atoms, must likewise be elastic; that is, they must be soft; for elasticity, according to the ideas we have of it, is nothing but active softness. Therefore, it appeared to me that the ultimate atoms ought to possess two properties in direct contrariety, hardness and softness, which is manifestly impossible.

Having arrived at this conclusion, which appeared to render the probability of success of all future inquiries in this track desperate, it might be supposed that my efforts would have ter-

minated. Nor, perhaps, is there any thing that could have induced me to perplex myself any more with this subject ; but the resolution I had formed to examine it thoroughly, and a fondness for the plausibility of my preconceived notions that I could not shake off, and which would oftentimes, even against my inclination, prompt me to try to explain away the absurdity I had brought out. The first thing that suggested itself for this purpose was, that elasticity might spring from a different source to what was commonly believed, and might be the property of hardness ; for I observed that the harder the bodies are, generally speaking, the more elastic they are. Thus glass is very hard, and likewise very elastic ; and the same is true of steel, and most of the other metals. Upon this hypothesis, therefore, I now tried to investigate the laws of gaseous bodies ; and as far as I then carried my speculations, the conclusions I drew exactly coincided with phenomena. But reflecting more deeply on the subject, I convinced myself that, however well these inferences and phenomena might agree, elasticity could not be a property of hardness ; and, therefore, that the hypothesis I had assumed could not be correct. At length, after a great deal of intense and fruitless thought, I remembered that when, some years before, reading the vulgar doctrine of the collision of hard bodies, I was very far from being satisfied with it ; but looking upon it then as an abstract and almost useless subject, I could not summon resolution enough to give it a critical investigation. Being now, however, drawn to the point by my analytical inquiries, the recollection of this dissatisfaction excited me to consider the circumstances connected with it attentively. The result of this consideration was a theory for the collision of hard bodies, so very different from the received theory, that it was not until I had examined it in a variety of shapes, had brought it to the test of experiment by my mathematical investigation of the laws of gaseous bodies, and had found that a theory something like it had been formerly given by Wren and Huygens, that I could satisfy myself I had not committed some oversight. But having considered and reconsidered it many different ways, without discovering any thing that could militate against it, I proceeded to carry on my theory as far as I judged it would be wanted, and then assumed the following postulata as the basis of my future inquiries.

Postulata.

1. Let it be granted that matter is composed of inert, massy, perfectly hard, indestructible atoms, incapable of receiving any change or impression in their original figure and nature.
2. Let it be granted that all solid and fluid bodies have their smaller parts composed of these atoms, which may be of different sizes and figures, and variously associated, according to the manner which the constitution and nature of the bodies require.

3. Let it be granted that gaseous or aeriform bodies consist of atoms, or particles, moving about, and among one another, with perfect freedom.

4. Let it be granted that what we call heat arises from an intestine motion of the atoms, or particles, and is proportional to their individual momentum.

5. Let it be granted that a gaseous body of very great tenuity in its parts fills all space, and extends to its utmost limits.

I have purposely put these hypotheses (if indeed we can call those things hypotheses which have been deduced from the analysis of phænomena) into the form of postulata, to avoid being obliged to establish them by direct demonstration. It is not my intention, for the reasons I have already given in the beginning of this memoir, to make any comparative remarks on their relative simplicity and probability. I shall only say a few words for the purpose of explaining the difference between my views on certain points and those which have been taken by others.

One of the sublimest ideas of the ancients was, that there is but one kind of matter, from the different sizes, figures, and arrangements of whose primitive particles, arises all that beautiful variety of colour, hardness and softness, solidity and fluidity, opacity and transparency, &c. which is observed in the productions of nature. Our first two postulata do not necessarily require that there should be but one kind of matter; there may be several kinds. But since it seems possible to account for all the phænomena on the supposition of one kind only, and since nature is always disposed to employ the simplest machinery, probability is strongly in favour of the ancient idea. In fact it does not seem to be impossible, from some of the phænomena of light and other circumstances, to show that nature has embraced the simplest means, and has likewise, if not in the size, at least in the figure of the atoms, confined herself within certain limits. But these things are too recondite to be pursued in this memoir; and experiments have not yet furnished us with sufficient data to be able to exhibit the exact line and rule with which nature has laid out her work.

Philosophers, since the time of Newton, have taught us that the elasticity of gases is owing to a mutual repulsion between their particles, by which they endeavour to fly from one another; but by our third postulatum we have divested matter of this repulsive property, and nevertheless, as it will be seen, the laws of gaseous bodies, investigated under this point of view, agree mathematically with phænomena.

The advocates for the theory of heat by intestine motion have usually considered the temperature as measured by the velocity of vibration; and I am not aware that any of them have defined it otherwise. This will do very well for different temperatures of the same body; but it seems to require the theory I have given in the fourth postulatum to enable us, under all circumstances, to compare the temperatures of different bodies.

In the fifth postulatam I have given the fluid of Newton for explaining the cause of gravitation. This illustrious philosopher has so clearly developed his ideas of the nature and action of this æthereal fluid, that I have had scarcely any thing to do but to confirm them with the application of the principles of our third and fourth postulata. It is true that the novelty of the views I have been obliged to take, and the unbeatenness of the track, have rendered even this a task of some difficulty; but the results I have obtained will, I presume, convince the Royal Society that my efforts have not been wholly unsuccessful; and that this idea of Newton, which has, from the uniform want of success to demonstrate it, often been placed to the account of this great man's foibles, was not adopted upon light grounds, or without mature consideration.

OF THE COLLISION OF PERFECTLY HARD BODIES.

Definitions of Hardness, Softness, and Elasticity.

Def. 1.—That body is perfectly hard whose figure cannot be altered by any weight, or percussion.

Corollary.—Hence a perfectly hard body must also be perfectly entire; for if it be composed of parts, there may be a force sufficient to separate them, and then the figure would be changed, which is against the definition. By a hard body, I mean one without parts, unchangeable, and indivisible, such as, perhaps, the primary particles of matter are.

Def. 2.—The figure of a soft body yields to pressure, or percussion, without recovering itself again.

Cor.—Hence a soft body cannot be entire, but must be composed of parts, which, being displaced, retain whatever situation is given them.

Def. 3.—A perfectly elastic body, like a soft one, suffers its figure to be changed by force, but recovers it again with an energy equal to the force by which it was changed.

Cor. 1.—Therefore an elastic body does likewise consist of particles, which, like the particles of a soft body, may be deranged; but as soon as the power is overcome by which they were disturbed, they exert as much force in recovering their situation, as was used in depriving them of it.

Cor. 2.—Because an elastic body recovers its figure with the same force by which it was changed, as much motion is generated in the recovery as was destroyed in the loss of the figure.

PROP. I.

If two bodies absolutely hard impinge on one another, the duration, or smartness, of the stroke, is independent of the velocity of the contact; that is, it is neither augmented nor diminished by any increase or diminution of the relative velocity of the bodies.

For the bodies being absolutely hard, their figures do not yield to any stroke; however great it may be, and, therefore, the shock is no sooner given at one part than it is equally felt at every other; that is, supposing the stroke is given in the direction of the centre of gravity. Therefore, the stroke can have no duration, and consequently no increase or diminution of velocity can produce a difference.

The same thing might also be proved thus: Let two unequal hard balls, moving with equal momenta in opposite directions, be conceived to come in contact at the same time with the opposite parts of another hard ball at rest, then will the intermediate ball remain at rest, and not titubate the one way or the other, nor be any more affected than if it had not been struck at all; for since the contacts are made at the same time, if the intermediate ball titubates, that ball towards which it moves must take a longer time to give its stroke than the other, and cannot have completed it until the titubation is destroyed, because, as soon as the strokes are finished, the intermediate body evidently becomes quiescent in consequence of the assumed equality of the momenta; but the balls being absolutely hard, their figures do not yield to the stroke, and, therefore, this exterior body itself, towards which the intermediate one titubates, before it has finished its stroke, must, on the supposition of titubation in the middle ball, have moved backwards; that is, before it has finished the stroke, it must have had all its momentum destroyed, and a new contrary one generated. But the whole stroke which the ball could give consisted in its momentum; consequently the body must have given its entire impulse before it has completed it, which is absurd. Therefore the intermediate body does not titubate, and the strokes are made equally smart or in equal portions of time, or rather both strokes are made in portions of time which have no duration.

By deducing the collision of hard from that of soft bodies, we likewise arrive at the same conclusion. At the moment two soft bodies come in contact, the anterior parts of the second body communicate motion to the posterior parts of the first body, and a slight check is given to the second. The parts of the bodies yielding to the blow, the bodies themselves approach nearer. In the second instant another acceleration and retardation take place, and the bodies approach still nearer. The same things ensue in the third and succeeding instants, until at length the second body has itself lost and communicated to the first body a sufficient degree of motion to enable it to move with the same velocity which the second body has then left; after which the stroke ceases, the two bodies are at their least distance, and go on together. Thus it is with the collision of soft bodies, and those whose figures are yielding, except, perhaps, that the strokes are given in continued unceasing pressure, and not in a succession of impulses at stated intervals; but in whichever way it be conceived to be done, it amounts to the same thing, and

284 *Mr. Herapath on the Causes, Laws, and principal* [APRIL, time is always consumed. If now, other things being alike, we suppose the bodies to increase in hardness, then, since their parts have a less disposition to yield to the force of percussion, the intensity of the impulses (if we suppose the stroke to be given in impulses) will be stronger, the quantity of motion communicated by each greater, and the time of the whole stroke, therefore, shorter. And if we suppose, *ceteris paribus*, the hardness to increase still more, the duration of the stroke will be still less, until, if the hardness be perfect, there will be no yielding of figure, and no duration for the strokes. And since this is the case with every stroke between perfectly hard bodies, it follows that all the strokes between bodies absolutely hard have no duration, and are thence equally smart.

Cor.—Hence we gather, that in perfectly hard bodies, the intensity of the impulse depends on the violence or momentum, of contact, and is independent of the velocity of contact, except inasmuch as it is augmented or diminished by that velocity.

PROP. II.

If a hard spherical body impinge perpendicularly on a hard fixed plane, the body will, after the stroke, remain at rest on the plane.

For the plane having no motion of its own, and being fixed, the force with which the bodies come in contact will be equal to the momentum of the ball; and because action and reaction are equal and contrary, this momentum is the force with which the ball acts upon the plane, and the plane reacts upon the ball at the instant of contact. The force, therefore, with which the ball is acted on by the plane at the time of the contact in a direction opposite to its motion is just equal to its momentum; consequently the momentum and action of the plane being equal and opposite destroy one another; and the ball having no other tendency continues at rest on the plane.

Cor. 1.—Hence a hard ball impinging obliquely upon a fixed hard smooth plane slides along the surface of it in a determinate direction with a determinate velocity. For if the motion of the body previous to the contact be resolved into two, one perpendicular and the other parallel to the plane, the perpendicular part will be entirely destroyed by the contact, but the other part being that with which the body would neither recede from, nor approach the plane, will continue the same after as before the stroke, and will induce the body to slide along the surface of the plane in its direction, and with its entire force.

Cor. 2.—From this proposition it appears that if, instead of the plane, the body meets with another equal and hard body moving equally in an opposite direction, the intensity of the stroke will be twice as great as between the body and the plane; for the plane being fixed contributes nothing to the violence of the blow, but the other body coming with an equal force in a contrary direction, adds its whole motion to the force with which

the other body would have come in contact with the plane, and, therefore, makes the stroke twice as great.

This might probably be made more obvious thus : Suppose a hard plane, or other body, be held against a fixed hard body, and in this way receive the impulse of the ball ; then, because that part of the intermediate body which is against the fixture is not urged any way by that fixture, the force with which the ball comes in contact with the other side is the force with which the sides of this intermediate body are driven together ; but this force is the momentum of the ball ; therefore, that momentum is the force of constipation in this case. But if we now fix the intermediate body, and instead of the fixed body on one side of it imagine another equal ball to come in contact with it at the same time as the former, and with an equal momentum, then the force with which each surface of this intermediate body is urged towards its centre is equal to the momentum of each of the balls ; and, therefore, the force with which the two surfaces are urged together is equal to the sum of these momenta, or to twice one of them ; but this force is manifestly the force with which the two balls would have come in contact if there had been no intermediate body ; therefore, that force is the double of the force with which either body would have struck a fixed plane.

Cor. 3.—Hence if two hard and equal balls come in contact with equal and opposite momenta, they will separate after the stroke with the same velocity with which they met. For since the intensity of the stroke is the force with which each of the balls is acted on in a direction opposite to that in which it came at the time of the contact ; and since that intensity is by the preceding *cor.* equal to twice the momentum of either ball, each ball at the time of the contact might be conceived to be acted on by two opposite forces, one its momentum, impelling it towards the other ball ; and the other, the force of the contact equal to twice its momentum impelling it in an opposite direction. The difference between these two forces, therefore, or the value of one momentum, is the force with which each ball retraces its path ; and, consequently, the velocity of the separation of the balls is equal to the velocity of their approach. This coincides with the theories of Wren and Huygens.

Scholium.

By the old theory of collision, two hard bodies coming in contact with equal opposite momenta will not separate after the collision, but will continue together ; and the reason assigned for this is, that being unelastic, they cannot, when they meet, exert themselves to separate, and, therefore, must remain together. Such a method as this is not reasoning from the property of hardness, the physical force of the impulse, and the effect which that force would have upon the motions of the bodies ; but from the absence of a property which does not belong to this class of

226. *Mr. Herapath on the Causes, Laws, and principles* [As res, bodies, but to one whose nature is so very different, as to be almost the very opposite. Hardness and softness are diametrically opposite properties; and elasticity is nothing but an active kind of softness; for elasticity consists in a vigorous restoration of an altered figure; and no body can have its figure altered which is not more or less soft. To argue, therefore, that two hard bodies which meet each other with equal and contrary momenta cannot separate after collision, because they have no elasticity, is evidently to abandon the definition of hardness, and to adopt that of elasticity, which has no connexion whatever with it, and consequently ought, in such a case, to be excluded. It is no matter of surprise, therefore, with such incongruous ideas, that mathematicians have hitherto had erroneous views of the theory of collision of hard bodies. Probably the apparent sterility, if not inutility of the subject, has occasioned an apathy towards a scrupulous investigation, which the slightest idea of its importance would have easily removed. Had it been imagined that the collision of hard bodies was connected with the development of the cause of heat, gravitation, light, magnetism, electricity, &c. it would have been scrutinized with a care which nothing could have escaped; and with a rigorous investigation, I am persuaded our ideas of the subject would have been very different to what they are.

If there be any method of classification which should have a preference, it appears to me it should be to rank all those bodies under one head which have mutable, and those under another which have immutable figures. To the latter class will belong hard bodies, and to the former every variety of soft and elastic bodies; the one will give their strokes instantaneously, and without the lapse of time; the other, gradually and with time. In each particular case, the physical nature of the impulse should be considered, and a theory of collision framed accordingly. With such views, our theories of collision would be made to rest on their true and veritable principles, the physical nature of the bodies and of the strokes which they give.

Many simple experiments might easily be devised to prove the truth of our second cor.; for it is immaterial on what bodies we experiment: we can draw the same inference for any. The thing, however, is so obvious that I have generally considered it in the light of an axiom; and have often ascertained the opinion of other people on the same subject by the following question: "Suppose a hard sphere, moving freely with a given velocity, strike directly upon a hard fixed body, it would strike with a certain intensity: but now suppose that instead of the fixed body the moving sphere strike upon another hard equal body, moving with an equal velocity, in an opposite direction, what would be the relative intensities of these two strokes?" The answer has invariably been, that the latter would be the double of the former. This, it must be allowed, is not a mathematical,

or even a philosophical way of establishing the question; but in a case of this kind, where I thought my prejudices might influence my judgment, it appeared no bad method of examining the soundness of my opinion by the standard of other people's.

From all these circumstances, it appears that the vulgar doctrine of the collision of hard bodies is, in this particular case, incorrect, by making the intensity of the stroke only the half of what it should be. For the bodies remaining together after the impulse, the force of the stroke upon each must be equivalent to the motion destroyed; that is, to the momentum of either of them. But the force upon each is the force with which they come in contact; the force, therefore, with which they come in contact is equal to the momentum of one of the balls; that is, agreeable to both theories, to the force with which either ball alone would, with the same momentum, strike a fixed plane.

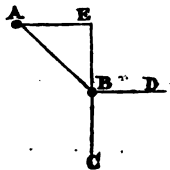
PROP. III.

If a hard ball strike another hard ball at rest in the line of their centres of gravity, an exchange of state will take place; the former will remain at rest after the stroke, and the latter will proceed in the same direction in which the first was moving, and with the same momentum.

If this be not the case, the first body must, after the impact, either move backwards or forwards in the direction of the other body, with an equal or less velocity. But it cannot move backwards, because the intensity of the stroke itself on a quiescent body can evidently never exceed the momentum; therefore, if it move at all after the stroke, it must follow the other body, with an equal or less velocity than this body acquires from the impulse. Suppose it be with a velocity b , either equal to, or less than, that acquired by the other body; and suppose A represent the first body, and a its velocity before the impact. Then because $(a - b)$, A is the motion lost by A , on account of the impact, and consequently the motion gained by B , the other body. This quantity represents the intensity of the impulse. And in any other case $(a' - b')$ A' represents also the intensity of the impulse; but if the quiescent bodies be equal, and if the momenta $A a$, $A' a'$, of A and A' , before the impact be equal, the strokes themselves, by cor. to prop. 1 will likewise be equal; that is, $a A - b A = a' A' - b' A'$, and, consequently, $b A = b' A'$; or the motion which is left to each of the bodies, A , A' , after the impulses, will be the same. Now whatever be the values of the momentum $A a$, if we imagine the body A to be vastly less than B , the velocity of B after the impulse must be vastly less than that of A before the impulse; and, therefore, the motion $b A$, which remains to A after the impulse must be vastly less than $A a$, the motion of A before the impulse. And if we suppose A so small as to have a ratio to B less than any assignable ratio, the ratio of $A b$; and, therefore, of $A' b'$ to $A a$, or $A' a'$, will also be less than any assignable ratio. Therefore,

if the ratio of A' to B be assignable, the motion of A' after the stroke will be unassignably small; that is, the body A will remain at rest. And because $b' A'$ is indefinitely small compared to $a' A'$, the intensity $a' A' - b' A'$ of the impulse will likewise be equal to the momentum $a' A'$ of the moving body before the stroke. But since the intensity of the impulse is the force acting upon the quiescent body at the time of the impulse, it is also equal to the motion acquired by this body. Therefore, if a hard ball strike another hard ball at rest, &c.

Cor. 1.—From this proposition it is easy to determine the motion and direction of a hard body striking obliquely with a given momentum in a given direction on another hard body at rest. For if AB be the direction and momentum of the body previous to the stroke, and BC the direction in which it strikes the quiescent body B , produce CB to E , on which demit the perpendicular AE , and draw BD equal and parallel to AE , and BD will be the motion and direction of A after the stroke, and BC , if equal to EB , those of B .



Cor. 2.—Hence it follows, that in any oblique collision on a quiescent body, the motions of the bodies after the impact will be perpendicular to each other.

Scholium.

I forbear to enter further into the collateral minutiae of this theorem, because it would lead me too far out of my way, and I am in haste to arrive at things of more importance. However, it is necessary to state that I have chosen this indirect method of demonstrating this proposition, for the sake of making it rest on principles as different and as independent as possible of those of a future proposition, from which it will flow as a corollary.

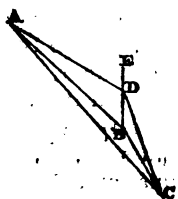
PROP. IV.

If a hard body overtake and strike another hard body, moving with a less velocity in the same right line, the first body will, after the stroke, continue its course with the same velocity which the other body had before it; and the second body will acquire from the stroke a momentum equal to the difference of the velocities of the bodies previous to the contact, drawn into the mass of the first body; that is, if A, B , represent the two bodies, and a, b , their velocities before collision, the motion of A afterwards will be $A b$, and that of B , $B b + (a - b) A$.

Because the bodies are both moving the same way, it is evident that we may conceive the second body to be at rest, and the other body to strike it with a velocity equal to the difference of the velocities a and b ; in which case the proposition will come to the same thing as the last. Therefore $(a - b) A$ is the momentum, or force, of collision; and is, consequently, the

motion acquired by B and the motion lost by A. Whence the motion of A after the stroke is $Aa - (a - b)A = bA$, and that of B is $Bb + Aa - Ab$.

Cor. 1.—By this proposition, the direction in which a body overtakes and strikes another being given, as well as the motions and directions of the bodies before the stroke, the motions and directions of the bodies after the stroke may be found. Let AB be the motion and direction of the body A before the impact, and let BC be the same of B, and let EB be the direction in which the impulse is made by A, and DB the intensity of it, or the quantity of motion with which A in the line EB overtakes and strikes the body B. Then join AD and DC, and they shall be respectively as the quantities of motions and directions of the bodies A and B after the stroke.



Cor. 2.—Draw AC; then, since AC is the motion compounded of the motions AD and DC, and likewise of AB and BC, it follows that the aggregate motions of the bodies before and after the stroke, reduced to the same direction, are the same: and, consequently, the motion of the common centre of gravity of the bodies remains unaffected by the impulse.

PROP. V.

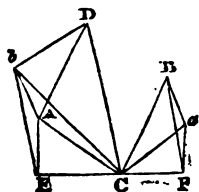
If two perfectly hard bodies, moving in the same right-line but towards opposite parts, come in contact, an exchange of motion will take place; or each body will retrace its path with the motion which the other had before the contact.

Let A and B be the two bodies, moving in opposite directions with the velocities a and b . Then, because Aa is the motion with which A advances towards the parts B is leaving, and Bb is the motion with which B advances towards the parts A is leaving, the sum $Aa + Bb$ of these momenta is the motion with which the two bodies approach; and, therefore, the motion, or force, with which their surfaces come in contact. But the force with which the surfaces come in contact is the force with which each surface, or body, is acted on at the time of contact in a direction opposite to that in which the body was moving. Therefore, at the time of contact, each body is acted on by two opposite forces; one its momentum; and the other, the force of contact, or the sum of the momenta of the two. Consequently, the difference between these forces, or the momentum of the other body, is the motion with which either of them is impelled backwards, and retraces its path after the stroke.

Cor. 1.—Hence if one of the bodies be at rest before the stroke, the other will be at rest afterwards; and that which was at rest will go on after the stroke with a motion equal to what the other had before. These things coincide with what we have deduced in our third proposition; but the proof here given is

much more direct and rigorous than the one in that proposition; for which reasons it will enable us presently to consider one or two points of our theory that we could not before, at least in the manner we are now enabled to.

Cor. 2.—The motions and directions of two balls being given, and the direction in which they strike one another being also given, the motions and directions of them after the stroke may be found. Let AC , BC , be the two given motions previous to the contact, and let ECF be the direction in which the balls strike. Upon EF let fall the perpendiculars AE , BF ; and from the points E , F , draw Eb , Fa , respectively equal and parallel to FB , EA ; join aC , bC ; and aC will be the motion of A , and bC that of B after the impulse.



Cor. 3.—From this cor. it follows, that the compound motion of the bodies is the same before and after the impulse. For draw AD equal and parallel to CB , and join DC , which will be the compound motion of the bodies before the impulse. Likewise draw bA , Ba , and bD . Then because EA , Fa , and Eb , FB , are equal and parallel, Aa , bB , are equal and parallel; and because AD , CB , are equal and parallel, bD , Ca , are also equal and parallel; but by the preceding cor. aC is the motion of A , and bC that of B after the stroke; DC is, therefore, the motion compounded of these motions. The same DC has also been shown to be the motion compounded of the motions of the bodies before the stroke; whence the motion compounded of the motions before the stroke is the same as the motion compounded of the motions after the stroke. Consequently, the motion of the common centre of gravity of the bodies receives no change from the collision.

Cor. 4.—The same inferences that we have drawn in the preceding cor. might have been easily drawn from other premises. For since action and reaction are equal and contrary, the motion of each body is equally affected by the stroke; and whatever is gained by the one in any direction is lost by the other in the same direction; so that the aggregate motion of the bodies in any direction is always the same, unless some extraneous force interferes.

Scholium.

Having now brought our theory of collision as far as it will be wanted in the subsequent part of the memoir, I shall omit the more intricate problems connected with it, and shall only stop, before I proceed to the theory of gases, to consider another error in the old theory, and to clear up one or two points in the new, where, I think, from the novelty of the views and a natural prejudice in favour of preconceived notions, objections may arise.

According to the commonly received theory of collision, when a hard body strikes another at rest, the two bodies proceed together with a common velocity. Now if these bodies have no kind of attraction, they do not press after collision; and, consequently, either of them may be taken away without affecting the motion of the other. Hence the motion which the body that was quiescent has acquired is proportional to the intensity of the impulse, and may be taken as a measure of it. Therefore, if the body A with a velocity of a strike the quiescent body B, the velocity of the balls after the stroke will, by the old theory, be $\frac{Aa}{A+B}$, and

the intensity of the stroke $\frac{aAB}{A+B}$. The ball B and other things remaining the same, if, instead of A we substitute a ball n times greater, the intensity of the stroke would be $\frac{naAB}{nA+B}$; and there-

fore the ratio of these two strokes is that of $nA+B$ to $n(A+B)$. That is, if two perfectly hard balls strike similarly and with equal velocities, two similar, hard, and equal balls, B, B, at rest, the ratio of the strokes will be that of $nA+B$ to $n(A+B)$. But by our first cor. to the preceding prop. the ratio of these intensities should be that of 1 to n ; and the same is true by the third prop. The ratio of the strokes, therefore, as given in the two theories, differs materially. When the ratio of the stroke of a less to that of a greater body, under the same circumstances of action is considered, it is greater in the old than in the new theory, and conversely; and in all cases, except one; namely, the collision on a fixed plane, the intensity of the blow is less in the old than in the new theory. It is not an easy thing to examine the truth of either of these theories by direct experiment; except, perhaps, in the case that I have mentioned in the scholium of prop. 2, for want of perfectly hard bodies to experiment on; but probably, in the absence of experiment, the perfect coincidence of phænomena, and the consequences that I have drawn, from these new principles of collision in the following theory of gaseous bodies, will be admitted to amount, as nearly to an experimental proof as the nature of the subject allows.

Besides the methods we have adopted to consider various points in the collision of hard bodies, there are several others, all of which, however, come to the same thing; but the following method of examining the case of this schol. which has occurred to me while I have been writing this part of the memoir, appears to be so independent of previous considerations that I have been tempted to give it. Setting aside all idea whether the bodies after collision do or do not continue together, let us only suppose that they are absolutely hard, that the two quiescent ones are perfectly equal, and that the other bodies before the strokes have equal velocities, and move similarly upon the quiescent

bodies. Then because the bodies are perfectly hard, the strokes will be equally diffused, and felt in every part of the impinging bodies; and, therefore, every part of the impinging bodies will equally contribute to the stroke. And the same things will evidently hold good if each impinging mass, instead of being one entire body, be composed of two or more moving along in contact with a common velocity, provided the centres of gravity of all the bodies and their points of contact be all in the line in which the impulse is given. Again, because the bodies struck are equal and similar, and the strokes are made similarly and with equal velocities; the strokes, as far as they depend upon these circumstances, must be identically the same. Therefore, whatever be the difference in the intensities of the strokes, it is wholly attributable to the difference in the masses of the impinging bodies. But we have already shown that the stroke is the same, under certain conditions, whether the impinging body be one or several bodies in contact. If, therefore, we conceive the greater impinging body to be composed of two, one of which is equal to the other impinging body, then, since the mere contact of the two parts can have no influence in augmenting or diminishing the intensity of collision due to either of them separately, the intensity of the impulse of the other body, and of the part which is equal to it, are consequently equal. But because every part of the impinging body equally contributes to the stroke, the intensity of the impulse due to a part, whether that intensity be equivalent to the whole, or only to a portion of the momentum, is to the intensity due to the whole of the body as the part is to the whole. The ratio, therefore, of the impulses is equal to the ratio of the impinging bodies.

When a hard ball strikes another hard ball at rest, in the line of its motion, the effect of the collision is a mutual change of state. And since by cor. 1 to the preceding prop. this is true without regard to the relative masses of the balls: it follows that a body in a state of free and perfect quiescence, however small it might be, will destroy the motion of another body however large and however great its momentum. Thus then a single particle of matter, of the smallest dimensions, to which a very small force would give a velocity sufficiently great to avoid a stroke from a very large body, moving with a much greater momentum, may, if struck, when at rest, stop another of any dimensions and moving with any force. This conclusion, which, at first view, appears to throw an air of improbability over the theory, will, upon a closer inspection, be found to be perfectly natural and correct. For the effect in motion on either of the balls is equal to the intensity of the impulse, and that intensity, by the aforesaid cor. and by a variety of other considerations which it would be tedious to state, is equal to the momentum of the moving body. It is, therefore, not on the *relative magnitudes* of the bodies that the change of motion depends, but on the

momentum of the moving body before the contact; so that the effect of a very large body moving with a less velocity, may be equal to the effect of a very small body moving with a greater velocity. Hence the whole difficulty of this case turns upon the abstraction of the ideas of magnitude and momentum; and, therefore, if we admit a reciprocal change of state when the balls are about equal, we cannot refuse it in any other case.

Again, it has generally been admitted that the relative motions of bodies, included in a given space, and their mutual actions on one another, are the same, whether that space be at rest, or move uniformly forward in a straight line. This is true with elastic and soft bodies, and also with hard ones when they are equal; because then their relative velocity is the same before and after the stroke; but when they are unequal, it is very different. In cases where the masses are very unequal, the difference in the two results will, in general, be very great. Let a body whose mass is 8 and velocity 6 strike another at rest whose mass is 2; then the velocity with which these bodies separate after the stroke is 24. But if we estimate this relatively to a space moving with a velocity of 4 in the same direction, it becomes 9, which is but little more than a third of the velocity, with which the two bodies do really separate. And the same anomalies might be shown to exist in other cases of this theory with respect to the collisions of unequal bodies under similar circumstances. It is, therefore, by no means immaterial, as it has generally been imagined (Newton, cor. 5, of the third Law of Motion), whether we calculate the effects of collision according to absolute or relative rest; the substitution of the one for the other might produce very erroneous results. These considerations, however, will not at all affect the validity of our deductions in the laws of gaseous bodies. For the particles moving and striking in all directions, whatever force is gained by relative motion in the one is lost in the opposite direction; so that the mean force, which is all that we consider, will be the same in both cases

(To be continued.)

ARTICLE IX.

On the Comparative Temperature of Penzance. By Dr. Forbes.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Penzance, Feb. 3, 1821.

IN the small tract lately published by me on the Climate of Penzance, I have pointed out the relative temperature of a

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 variety of places in the island ; all which observations illustrate, in a very striking manner, the effect of the *peninsular position* of this place in *equalizing* its temperature. In further illustration of this fact, I subjoin the principal results of the last three months at *Penzance* and *Edmonton* (Middlesex), as procured by the register thermometer. On this occasion I would beg leave to suggest the advantage of collecting the comparative observations made by register thermometers throughout the kingdom, and of publishing them monthly, condensed into a comprehensive and manageable compass, as in the following table. If you approve of the proposal, and will solicit the assistance of observers in different parts of the kingdom, I have no doubt but this will be cheerfully granted. By such a plan as this, I am convinced a much more clear and exact knowledge of meteorology would be diffused in a few years than by the long continued publication of the voluminous and unconnected diaries at present diffused through various journals. If your friend Mr. Luke Howard, who is, perhaps, better qualified for the task than any other person, would undertake to construct a plan, and submit it to the public, I have no doubt of the success of the measure. Of course, in this it would be necessary to include all the more important features of the science. In the following table I confine myself to the *temperature*.

I am, Sir, your obedient humble servant,
 JOHN FORBES.

Comparative Temperature of Penzance, in Cornwall and Edmonton, Middlesex, in Nov. and Dec. 1820, and Jan. 1821, by the Register Thermometer.

	November.		December.		January.		
	Penz.	Edm.	Penz.	Edm.	Penz.	Edm.	
Absolute maximum.	56°	58°	54°	56°	53°	53°	
Absolute minimum.	35	22	21	21	26	21	
Mean of maxima.	50	47	46	43	47	41	
Mean of minima	44	35	40	35	41	33	
Mean of maxime and minima. ..	47	41	43	39	44	37	
Extreme monthly range	21	36	33	35	27	32	
Diurnal range {	Max.	13	25	14	19	12	21
	Min.	1	3	1	2	2	2
	Med.	6	12	5	8	6	9

N.B. The results for Edmonton are extracted from the diary of Mr. Adams, published in the Literary Gazette.

ARTICLE X.

Meteorological Results of the Atmospheric Pressure and Temperature, Rain, Wind, &c. deduced from Diurnal Observations made at Manchester, in the Year 1820. By Thomas Hanson, Esq. Surgeon.

Latitude 53° 25' North; Longitude 2° 10' West of London.

1820.	BAROMETRICAL PRESSURE.						TEMPERATURE.				RAIN.			WIND.																
	Mean.	Highest.	Lowest.	Range.	Greatest variation in 24 hours.	Spaces in inches.	No. of changes.	Mean.	Highest.	Lowest.	Range.	Greatest variation in 24 hours.	Manches.		Inches.	Ardwk.	Lymn.	Crumpsall.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.	Britak.	Boisterous.	
													Inches.	Wet days.																
January....	29.72	30.64	28.60	2.04	1.80	5.5	8	32.69	53°	13°	40°	19°	1.075	7	1.555	1.615	—	—	0	2	7	6	4	5	0	2	5	1	1	1
February....	29.82	30.12	29.34	0.78	0.55	3.0	10	39.9	53	26	27	15	1.520	11	1.632	1.419	—	—	0	6	0	9	4	7	0	1	2	3	1	1
March....	29.76	30.30	28.80	1.50	0.46	4.9	6	43.1	59	27	32	20	1.650	8	1.426	1.075	—	—	0	1	8	0	5	4	7	4	2	1	1	1
April....	29.80	30.46	29.12	1.34	0.44	3.7	9	50.7	67	34	33	21	2.380	19	3.282	2.147	—	—	0	2	1	0	4	15	3	4	1	0	0	0
May....	29.59	30.18	29.06	1.12	0.60	3.5	6	53.8	77	42	35	23	5.910	21	5.812	4.964	—	—	0	4	0	1	0	23	0	2	2	0	0	0
June....	29.79	30.30	29.14	1.16	0.30	3.0	8	58.6	83	44	39	21	2.790	17	4.198	2.265	—	—	0	1	0	1	0	9	10	7	2	0	0	0
July....	29.81	30.10	29.30	0.80	0.40	2.4	10	62.8	81	47	34	23	1.440	13	2.228	3.384	—	—	0	0	0	8	5	12	4	1	1	1	1	0
August....	29.72	30.12	29.37	0.75	0.43	3.0	7	60.7	76	43	33	23	3.650	20	4.232	3.178	—	—	0	1	0	0	1	25	1	1	2	1	0	0
September..	29.80	30.20	29.30	0.90	0.41	2.4	12	56.1	75	39	36	25	2.440	15	3.086	3.137	2.870	0	3	0	0	4	15	2	6	0	1	0	0	0
October....	29.13	30.45	28.45	2.00	0.85	5.6	15	47.7	61	55	26	14	4.310	21	3.944	3.228	4.410	0	6	0	1	1	19	5	1	4	0	0	0	0
November..	29.73	30.18	29.28	0.90	0.42	3.8	11	43.7	58	27	31	13	2.175	16	2.412	1.645	2.770	0	6	3	10	4	1	3	3	0	0	0	0	0
December..	29.83	30.15	29.40	0.58	0.36	2.9	10	42.0	57	29	28	14	2.850	13	3.694	1.802	4.140	0	8	3	4	12	2	1	0	1	2	0	0	0
Annual																														
Means, &c.	29.70	30.27	29.10	1.15	0.52	43.7	112	49.3	66	34	33	19	32.190	181	37.501	30.159	14.190		1.41	17	34	46	122	463	138	13	98	13	3	3

The annual mean temperature is nearly $49\frac{1}{4}^{\circ}$, being a little more than 1° less than the mean of last year. The mean of the first three months, 38.5° ; second, 54.4° ; third, 59.9° ; fourth, 44.5° ; of the six winter months, 41.5° ; six summer months, 57.1° . The maximum of 83° occurred on June 27, and the minimum of 13° on Jan. 1: difference of these extremes, 70° .

The mean annual pressure of the atmosphere is 29.70 inches; highest point, 30.64; which occurred on Jan. 8; lowest, 28.45, which was on Oct. 17: difference of these extremes, 2.19 inches. The mean daily movements of the barometrical oscillations measure nearly 44 inches. Total number of changes, 112.

The falls of rain, hail, snow, and sleet, during the past year, have measured a little more than 32 inches; which is something under an annual average. Very little rain fell during the first three months of the year; but the following month (May) was very wet, for there were nearly six inches registered. Upwards of four inches fell in October. Total number of wet days for the year, 181.

The reporter, as usual, has again to notice a prevalency of the south, south-west, and west winds. Out of 365 notations of the wind, 214 were noticed to blow from the above points.

There has been more thunder and lightning, during the former part of the year, than has been noticed in several former ones, particularly in the month of May. Lightning occurred on six days in that month, which were invariably attended with rain: sometimes it fell in torrents, and in three instances with hail.

My friend, Mr. Edward Stelfox, of Lymn, near Warrington, has favoured me with the above account of rain. Mr. S.'s rain-gauge is exactly the same as mine, and I can rely upon his account as correct. His annual register of rain, for the year 1819, was 29.305 inches; for the present, a little more than 30 inches. Mr. Stelfox noticed the temperature on Jan. 1, 1820, to be 13.5° ; on the 3d, at 13° ; and on the 22d of the same month at 10° .

The column of rain, headed Ardwick, has been furnished by my friend, Mr. John Dalton. His rain funnel is fixed about a mile out of Manchester, in an easterly direction, and is situated some little higher than mine. It has often been remarked, that Mr. Dalton's annual account invariably exceeds mine sometimes by five or six inches, as in the present instance. Mr. D. thinks that his funnel being larger may in part account for the difference.

However, I fancy, there is an error somewhere. It is much to be desired, that one uniform plan could be adopted with respect to measuring of rain. I have furnished a gentleman of Crumpsall, near Manchester, with a funnel, and the same means of measuring the rain as Mr. Stelfox uses: and, from his results, it appears, that our accounts pretty nearly agree.

Manchester, Jan. 17, 1821.

ARTICLE XI.

Register of the Weather for the Year 1820, kept at the Apartments of the Royal Geological Society of Cornwall, Penzance. By Edward Collins Giddy, Esq. Curator of the Museum. (Communicated by Dr. Forbes, Sec.)

1821.] *Mr. Giddy's Meteorological Journal for Cornwall.* 297

1820.	BAROMETER.*				THERMOMETER.				RAIN IN INCHES.				WINDS.																
	Max.	Min.	Med.	Greatest monthly range.	Greatest daily range.	Mean tempt. at Extremes.				Var. of temp.		Total.	Wet days.	Dry days.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Prevailing winds.	Brisk winds.	Boisterous winds.				
						1 a.m.	2 p.m.	7 a.m. and 2 p.m.	Max. at two.	Min. at seven.	Greatest monthly range.															Greatest daily range.	Mean daily range.		
January...	30.396	28.798	29.600	1.598	0.436	37°	41°	39°	56°	22	34	8	4	0.84	1.20	2.04	13	16	3	1	7	8	1	3	5	3	SE	7	5
February...	30.038	29.364	29.703	0.694	0.308	40	47	43	55	30	25	14	6	0.42	0.33	0.75	5	24	3	3	4	3	5	0	8	NW	6	1	
March...	30.050	28.790	29.691	1.260	0.480	42	50	46	62	30	32	19	8	0.54	0.07	0.61	7	24	3	4	3	2	6	7	3	W	4	1	
April...	30.110	28.912	29.678	1.198	0.372	48	57	52	64	40	24	16	9	0.55	0.04	0.59	7	23	5	2	1	6	0	1	2	12	NW	2	0
May...	29.996	28.931	29.521	1.065	0.506	54	62	56	68	46	22	14	8	1.22	0.46	1.68	19	12	1	9	6	1	0	17	4	2	SW	5	0
June...	30.026	29.322	29.807	0.698	0.273	58	67	61	72	50	25	14	7	0.54	0.26	0.80	7	23	6	0	0	5	1	1	3	14	NW	4	0
July...	29.948	29.104	29.669	0.844	0.330	61	66	63	72	58	16	10	6	1.31	0.50	1.81	7	24	3	1	5	1	1	0	9	SE	1	0	
August...	29.951	29.528	29.632	0.623	0.313	59	65	61	70	50	20	12	6	0.63	0.66	1.29	15	16	4	3	0	2	2	5	3	SW	8	2	
September...	30.024	29.352	29.746	0.672	0.320	55	61	57	66	48	18	12	6	0.97	0.67	1.64	10	20	4	2	0	6	0	5	8	NW	7	4	
October...	30.309	28.694	29.424	1.615	0.635	49	54	51	60	44	17	12	5	1.32	1.33	2.65	17	14	1	0	5	6	1	3	7	8	NW	6	9
November...	29.949	29.127	29.571	0.824	0.568	46	50	48	56	38	18	10	3	0.66	0.37	1.03	12	18	0	6	5	4	3	5	4	NE	4	2	
December...	30.068	29.022	29.630	1.046	0.429	44	46	45	55	28	29	7	2	0.81	0.45	1.26	18	13	1	7	1	5	1	6	5	5	NE	8	1
Annual Means, &c.	30.396	28.694	29.613	7.011	0.635	49	52	50	75	23	24	12	6	9.81	6.34	16.15	137	229	34	30	37	60	15	63	48	79	NW	62	25

* The barometer is calculated at the mean temperature of 32°.

Remarks.

January.—The weather, in general, fine, with sharp frost; air very keen. Some heavy showers of rain and snow. A heavy gale of wind on the 26th and 27th.

February.—The weather very fine in general. Some showers of snow and hail. A little frost towards the latter end.

March.—Weather very fine. Some frost in the early part of the month. A few showers of hail, snow, and rain.

April.—The weather very fine and pleasant throughout, with the exception of three or four days in the early part of the month, which were showery.

May.—The weather during the greater part of this month was wet and unpleasant. Some hail showers towards the end.

June.—The weather, in general, very fine; 25th, 26th, and 27th, uncommonly hot and sultry.

July.—Weather, in general, very fine. Some very heavy showers of rain on the 12th and 17th. Thunder and lightning on the 11th.

August.—This month, in general, showery; but, on the whole, good harvest weather: crops abundant. Thunder and lightning on the 30th.

September.—The weather in the early part of this month very fine. During the solar eclipse on the 7th, the thermometer in the shade varied between 2° and 3°. On the 18th, the weather became rainy, cold, and boisterous, and continued so to the end of the month.

October.—This month exceedingly fine till the 14th, when the weather suddenly changed, and became very stormy, with heavy rain, which continued to the end.

November.—The weather during the greater part of this month was gloomy and damp. Some heavy hail showers on the 15th.

December.—This month was, in general, gloomy and damp as the preceding. On the 14th, hail; on the 26th and the 29th, snow. On the 13th, 25th, 26th, 28th, 29th, 30th, and 31st (a most unusual length of time for this place) frost. The air exceedingly keen.

E. C. G.

ARTICLE XII.

The New Comet. By William Burney, LL.D.

(To the Editor of the *Annals of Philosophy.*)

SIR,

Observatory, Gosport, Feb. 24, 1831.

A COMET made its appearance here last evening at 35 minutes past six o'clock, two or three degrees to the north of *Algenib*,

the last star in the wing of Pegasus. It was 32° to the east of the sun, and set with Saturn at a quarter past eight o'clock, about W.N.W. but was $18\frac{1}{2}$ to the north of that planet.

Its bright *nucleus* was surrounded by a diffused *coma* three-fourths of a degree in diameter, and its perpendicular train was nearly 4° in length when the coruscations were most vivid, through the upper part of which a small star of the sixth magnitude was perceived by the help of a telescope. This is unquestionably the same comet that M. Nichollet, of Paris, is said to have discovered in the evening of the 21st ultimo, in the constellation Pegasus, but which, to our knowledge, has not yet been seen by the English astronomers.

March 2, 1821.

The new comet has changed its position among the fixed stars about two-thirds of a degree, since the evening of the 23d ultimo, when first we saw it; therefore, its apparent motion is very slow.

Its north polar distance last evening was	$77^\circ 40'$
Right ascension	358 35
North declination.	12 20

It is descending with an antecedential motion towards the back of the southern fish of Pisces, and by the annual motion of the earth, the sun is daily approaching it.

It will come to its perihelion two or three days before the vernal equinox; but previous to that time, probably, it will be lost to our view in the solar rays in the evenings. It sets soon after eight o'clock, p. m. about W.N.W.

Its train last evening, at one time, was between 5° and 6° long, and will continue to increase in length and splendour till the comet comes to its nearest point to the sun; for then the heat communicated to it is increased by his proximity.

Neither the *nucleus* nor train is so splendid as the comets that appeared here in 1811 and 1819; it more resembles the comet in 1807, except the colour of the body, the present one being more brilliant.

March 17, 1821.

The new comet came to its perihelion to-day; namely, within 14° of the sun. It has only lessened its right ascension half a degree, and its north declination four-fifths of a degree since the evening of the 24th of February last, when it was first seen here; but by the annual motion of the earth, its distance from the sun is decreased about 16° . Now it is advanced too far in the solar rays to allow us to make further observations on its position in the evenings. At the close of this month, it will begin to set after the sun; and with a clear horizon an hour before sunrise, there will be a chance of seeing it rise about E.N.E. during the ensuing month.

The weather lately has been unfavourable for seeing the comet

so near the western horizon in the evenings ; and from its very slow geocentric motion, it will not afford a sufficient space to attempt to deduce the form of its orbit, which is the chief object to science.

It is hoped that correct observations on the frequent appearances and motions of those celestial visitors will, in the course of time, throw new light on the theory of comets, and divest it of much of the uncertainty that seems to exist in regard to the form of their eccentric orbits and periodic returns.

I am, Sir, your obedient servant,

WILLIAM BURNBY.

ARTICLE XIII.

On Oil and Coal Gas.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

ON perusing Mr. Ricardo's paper in your last number "On the Comparative Advantages of illuminating by Gas produced from Oil and from Coal," I felt a little surprised that he had not obtained more correct information respecting the value of coal gas ; for he states that the selling price of coal gas is not less, but "in some of the provincial towns and cities more than 15s. per 1000 feet." In speaking of the profits of Gas Companies, he says, "The highest return quoted, which is of the Bristol Gas Company, is 10 per cent. and the chartered Company in London, *which possesses advantages superior to any*, only divides eight per cent." Not knowing the price of gas at the chartered Company, I assume that it is 15s. per 1000 feet to the consumer. The Sheffield Gas Light Company retail their gas at 12s. per 1000 feet, and allow a discount of from 5 to 20 per cent. in proportion to the rental. All consumers of gas whose rental is 20l. per annum, or upwards, are allowed a discount of 20 per cent. When the discount is allowed, I believe the average to the whole town is at about 10s. 6d. per 1000 feet, and yet the Sheffield Gas Light Company has divided a *profit of 10 per cent.* the first year. This certainly proves that the Sheffield Gas Light Committee are either better managers of the trust confided in them, or that the chartered Company does not possess advantages superior to *any other* Gas Company. But what will Mr. R. say to the Derby Gas Company when I inform him that they charge only 7s. 6d. per 1000 feet, to the consumer ; but as their concern is only in its infancy, I cannot state their profits. If we allow that one foot of oil gas is equal in illuminating power to

three and a half feet of coal gas, the comparison between the Derby prices and Mr. R.'s statement of oil gas will stand thus :

	£	s.	d.
1000 feet of oil gas cost.	1	7	0
3500 feet of coal gas is sold at Derby for. ...	1	6	3
	<hr/>		
	0	0	9

leaving a balance of 9d. in favour of coal gas besides the profit, which the Company calculate at 10 per cent. The comparison between oil and coal gas by experimentalists in general is given as 1 to 3 : allowing this to be nearly the truth, we shall have the following statement :

Mr. Ricardo says that a fair average for

	£	s.	d.
1000 feet of oil gas, allowing for wear, tear, and every contingency, is	1	10	0
3000 feet of coal gas sells in Sheffield for. .	1	11	6

Now if we allow that oil gas can be manufactured at 30s. per 1000 feet, selling it at the same price as coal gas, in proportion to its illuminating power, it would only leave 5l. per cent. supposing that there should be no losses. Mr. R. further remarks, that "the only possible inconvenience that can result from the use of oil gas is an accidental smell from the cock being accidentally left open, which is instantly detected, and as instantly remedied without leaving so unpleasant a smell as that arising from the similar escape of coal gas." I think that there is very little difference between the smell of oil and coal gas as to pleasantness ; but allowing that the smell of coal gas is more unpleasant than that of oil gas when an escape takes place, that should not be urged as a reason why oil gas should have the preference ; for as both gases, when mixed with a certain proportion of atmospherical air will explode when a light is introduced, it is my opinion, that the gas most proper to be introduced in dwelling-houses, warehouses, &c. is that which can be most easily detected when an escape *does* take place.

If these remarks merit a place in your journal, your inserting them will oblige

A SUBSCRIBER.

ARTICLE XIV.

Researches on the Composition of the Prussiates, or ferruginous Hydrocyanates. By J. Berzelius.

(Continued from p. 232.)

I PROCEED to detail the experiments I have made with the design of obtaining light on these subjects, and the inferences which I think may be deduced from them.

I. *Researches on the Relation of the Iron to the other Bases in the ferruginous Hydrocyanates.*

(A.) *Salt with Base of Potash.*

The salt was purified in the following manner: Crystals that had effloresced were heated in close vessels until the mass entered into fusion. This was then dissolved in water, and twice crystallized. The solution was not rendered turbid by hydrocyanate of barytes, and the salt had a light, but pure lemon-yellow colour. It was pulverized, and left exposed to the air for two days without losing any weight. Exposed afterwards to the temperature of 140° Fahr. it effloresced, and lost between 0.129 and 0.124 of its weight: these numbers are the two extremes of seven experiments. It lost nothing further, at a temperature higher than that of boiling water.

I mixed 30.88 grs. of the salt thus dried, with sulphuric acid in a platina crucible; the mass heated, but without any effervescence, and no smell of hydrocyanic acid was perceptible. Heated on the sand bath, it sustained no change, and it was not, until it had been exposed to the flame of a spirit-lamp, that it began to be decomposed. A quantity of gas was then disengaged, which had not only the smell of sulphurous acid, but also that of the hydrocyanic. The heat was continued until the excess of sulphuric acid was driven off, and water was poured on the mass when cold. The sulphate of iron dissolved slowly; it was necessary to add water, slightly acidulated with muriatic acid, to obtain a complete solution.

The clear liquor was precipitated by caustic ammonia added in excess. The oxide of iron was thrown on a filter, and washed with a large quantity of water. The salt remaining in the solution and washings was evaporated to dryness, and heated red in a platina crucible. It is hardly possible to prevent a portion of the ammonia from being volatilized at the commencement of this operation, and consequently an acidulous sulphate of potash remains, which it is difficult to deprive entirely of its excess of acid. To get rid of this excess, I introduce a small piece of carbonate of ammonia, by a platina spoon, into the crucible, while the mass is still red hot. I then put on the cover to check the escape of the ammoniacal gas. The sulphuric acid almost instantly evaporates in the alkaline atmosphere, and the salt becomes neutral. In this manner I obtained in three separate experiments 29.11, 29.17, 29.49 grs. of sulphate of potash, equivalent to 51.09 hundredth of the weight of the anhydrous ferruginous prussiate. The oxide of iron weighed, in the different experiments, between 6.17 and 6.64 grs. Dr. Thomson's assertion, that a portion of the iron is volatilized with the hydrocyanic acid, induced me to try another method of separating it from this metal. I dissolved the ferruginous prussiate of potash in water, added hydrate of deutoxide of mercury to the solution,

and digested them together, till all the iron was precipitated in the state of red oxide. The excess of oxide of mercury was then separated by strong calcination : 30·88 grs. of the prussiate in question gave, in this manner, 6·42 grs. of oxide of iron. The mean result of these analytical experiments is, therefore, as follows :

			Oxygen.	
Potash	44·62	contains	7·58 2
Protoxide of iron.	16·64		3·79 1
Water	12·70		11·30 3
Loss	26·04			
	<hr/>			
	100·00			

It follows from this, that the potash contains twice, and the water three times as much oxygen as the iron, in the state of protoxide. The loss is nearly equal to the weight of four atoms of hydrocyanic acid ; but the supposition that the salt should contain this number of atoms is the less probable, because in that case, the weakest base, the protoxide of iron, must contain, relatively to its ordinary capacity of saturation, twice as much acid as the potash, the stronger base.

(B.) *Salt with Base of Barytes.*

This salt was obtained by digesting prussian blue with hydrate of barytes and water. The undissolved portion was treated with boiling water as long as it dissolved any thing, and the solutions were evaporated to a very small quantity. During the evaporation, the barytic salt crystallized irregularly, and it was necessary to redissolve it in boiling water, from which it separated on cooling, in the form of small yellowish crystals. It suffered no change at common temperatures, but at 104° Fahr. it began to effloresce. The crystals do not fall to powder, but only lose their transparency, and assume a milky appearance: 30·88 grs. of this salt lost, in this manner, in different experiments, from 5·09 to 5·12 grs. of water. I placed the remaining 25·76 on a small porcelain capsule, which I heated red by the flame of a spirit-lamp with a double current (*et à double courant*), and continued the heat till all the combustible matter was burnt away: the residuum was a mixture of barytes and red oxide of iron, weighing 24·63 grs. Dissolved in muriatic acid, and then precipitated by sulphuric acid, it gave 24·13 grs. of sulphate of barytes. After this was separated, I added ammonia in excess to the liquid, which threw down 4·09 grs. of red oxide of iron. According to this experiment, the barytic salt contains :

			Oxygen.	
Barytes	51·273	containing	5·38 2
Protoxide of iron.	11·865		2·70 1
Water	16·560		14·72 5½
Loss	20·302			

100·000

We find, therefore, the same relation between the protoxide of iron and the barytes, as in the salt with base of potash; on the other hand, the relation of the water to the bases exhibits, in this instance a remarkable exception to the ordinary rule, an exception that might very well be attributed to inaccuracy of observation, if the loss which this salt experiences by efflorescence were not so uniform and so easy to be correctly determined. When we compare the relation of the capacity of saturation of the bases, to the quantity of the substance lost in the analysis, with that observed in the salt with base of potash, we find, in the salt with base of barytes that the loss exceeds that in the salt with base of potash by a quantity precisely equal to that which the water wants (*que l'eau a donné en moins*) to make its oxygen *twice** that of the barytes, and six times that of the protoxide of iron. We shall soon see that this water is found again, when this salt is burned by means of oxide of copper.

(C.) *Salt with Base of Lime.*

This salt was prepared nearly in the same manner as the barytic salt. It is very soluble in water, and does not crystallize till the solution has acquired a syrupy consistence, and after some days rest. The crystals are usually large, and of a pale yellow colour.

One hundred parts of this salt, exposed to the heat of a sand-bath, lose 36.61 per cent. of their weight of water of crystallization. The crystals, notwithstanding this large quantity of water, do not fall to powder, and I have observed that although this salt begins to effloresce as readily as the preceding, a higher temperature is requisite to deprive it of the last portions of water. The anhydrous salt, burnt like the preceding in an open vessel, gave 50.53 per cent. of its weight of a mixture of oxide of iron, caustic lime, and carbonate of lime. It was dissolved in muriatic acid, perfectly neutralized by caustic ammonia, and precipitated by succinate of ammonia. The succinate of iron, burned on an open capsule of porcelain, left 15.25 parts of oxide of iron. The solution, from which the iron had been separated, gave, by precipitation by oxalate of ammonia, oxalate of lime, which, being decomposed by heat, left 39.21 parts of carbonate of lime. This carbonate, moistened with a solution of carbonate of ammonia, and then thoroughly dried, gained nothing in weight. The analysis, therefore, gave

Oxygen.				
Lime	22.45	containing	6.20 2
Proxide of iron. ..	13.69	3.12 1
Water	39.61	35.21 11½
Loss	23.85			

* Query, three times.

We find again here, as in the barytic salt, that half a proportion of water is wanting, that is to say, that the salt retains one atom of water; and by comparing the loss from the substances destroyed by heat, with the capacity of saturation of the bases, we also find in this case a surplus corresponding to the quantity of water that has disappeared.

(D.) *Salt with Base of Oxide of Lead.*

Neutral nitrate of lead was poured into a solution of ferruginous prussiate of potash, taking care that the latter should be in excess, in order to prevent the precipitation of nitrate of lead, which mixes with all the insoluble salts with base of oxide of lead, if there be an excess of nitrate of lead in the liquid from which they are deposited. The liquid remained perfectly neutral. The precipitate was white, but viewed in a certain direction towards the light, it appeared verging to yellow. I was unable to determine, with the precision I could have wished, the quantity of water contained in this salt, because its point of perfect dryness approaches too near to that at which it begins to effloresce. However, the results I obtained led me to believe that in this salt, just as in the salt with base of potash, the water contains the same quantity of oxygen as the two bases together.

One hundred parts of the salt, dried on a very hot sand-bath, were burned in an open porcelain capsule; the mass was dissolved in nitric acid, neutralized with ammonia, and precipitated by sulphate of ammonia. The sulphate of lead, washed, and calcined at a red heat, weighed 96.5 parts. The filtered liquid was precipitated by caustic ammonia, and gave 12.6 parts of oxide of iron. Thus the analysis gave:

Oxygen.

Oxide of lead	70.0	containing	5.09	2
Protoxide of iron.	11.9	2.57	1
Loss	17.7				

We find, therefore, also in this case, the same relation between the oxide of iron and that of lead, as well as between the bases and the substance destroyed by combustion, as in the ferruginous prussiate of potash.

I think these analyses, selected from the three classes of bases, will suffice to prove that whatever be the state of the iron in these salts, it takes, in the state of protoxide, half as much oxygen as the radical of the other base.

II. *Experiments on the Acid of these Salts.*

The proofs on which Mr. Porrett has founded his opinion, that the iron in the ferruginous hydrocyanic acid is in the metal-

lic state, and that it is a constituent part of this acid, are not decisive.

To verify this idea by direct experiment, I put 23·16 grs. of effloresced ferruginous prussiate of potash into a small apparatus, made by the lamp, and so disposed that a current of sulphuretted hydrogen gas might be passed over the salt. At the common temperatures it was not at all altered. I then heated it by a spirit-lamp, slowly increasing the temperature until the mass fused. The sulphuretted hydrogen gas was passed over it until the mass became cold. I had supposed that the sulphuretted hydrogen would decompose the protoxide of iron, producing hydrocyanic acid, water, and sulphuret of iron; but no trace of water appeared. The sulphuretted hydrogen, both at entering into, and leaving the apparatus, was passed over fused muriate of lime, in order that the water, that might be formed during the operation, might be correctly weighed. At the end of the experiment, the prussiate had gained 11·6 per cent. in weight, and the muriate of lime 21 hundredths of the weight of the prussiate. But on heating the muriate of lime, to see if what it had absorbed was water, it gave off only hydro-sulphuret of ammonia, and pure ammonia, with traces of moisture. No disoxidation of the protoxide of iron; therefore, had taken place, and the experiment appeared to confirm Mr. Porrett's idea. A great part of the salt had been converted into sulphuretted hydrocyanate of potash; a small portion remained undecomposed, and some sulphuret of iron was formed.

I next endeavoured to decompose the anhydrous prussiate by distilling it with fused boracic acid, to ascertain if, during this operation, borate of iron would be formed; but the mass swelled up greatly during the process, and passed out of the retort, so that its neck was soon stopped up by it. The disengaged gas was cyanogen, mixed with a little hydrocyanic acid. After the calcined mixture had been dissolved in water, a brownish mass remained, insoluble in muriatic acid, which, exposed to heat, gave borate of iron; whence it appeared that the boracic acid had been in part decomposed.

Thus the nonoxidated state of the iron seemed proved by these experiments. When I calculated the above-mentioned analyses, the result appeared to coincide with the following composition: one atom of metallic iron, two atoms of the oxide of the other bases, two atoms of cyanogen, and three atoms of prussic acid. It thus remained to determine by combustion, if that be the true state of the matter.

In consequence, I burned, in a glass tube surrounded by another of tin-plate, a mixture of 7·72 grs. of anhydrous ferruginous prussiate of potash, and 231·66 grs. of pure oxide of copper, prepared by the decomposition of nitrate of copper by heat. The mixture was made in a porphyry mortar, heated to above

212° Fahr. The gases were collected over mercury, and passed through tubes of very small diameters to prevent too great a mixture of atmospheric air. A portion of the gas obtained towards the end of the operation was collected separately in a graduated glass tube.

No moisture condensed in the tubes, in which, for greater certainty, a portion of fused muriate of lime was placed, and weighed with the tubes. They gained only 0.001 in weight. The gas collected in the graduated tube was examined in the following manner: a small piece of caustic potash attached to a very thin flexible iron wire was introduced into the tube; 100 parts of the gas left 39.9 parts not absorbed; so that the volume of carbonic acid gas was to that of the azote as 3 : 2. This result was very different from what I had expected. As no water was formed in the operation, it followed, that the salt contained n hydrocyanic acid. Mr. Porrett had found that the volume of carbonic acid gas was four times greater than that of the azote, and Dr. Thomson obtained them in the ratio of 2½ to 1. The whole of the carbonic acid obtained in this experiment, absorbed by a determinate quantity of potash, weighed 4.138 grs.

I repeated the experiment once more, but with a stronger heat, so as to soften the glass tube; I obtained the same result, but the relation of volume of the carbonic acid gas to that of the azote then exceeded the ratio of one and a half: however, the difference was so trifling that it might very well be only an error of observation.

Water digested on the residuum in the tube that had been exposed to heat, took up potash, and this solution gave an abundant precipitate with lime-water; the precipitate was carbonate of lime. To prove if the difference between my result and that of Mr. Porrett could be caused by a bad arrangement of my apparatus, I repeated the analysis of the cyanuret of mercury in the same manner; I obtained from it precisely two volumes of carbonic acid gas for one volume of azote; and on decomposing another portion of cyanuret of mercury by sulphuretted hydrogen gas, in a weighed glass bulb, I obtained sulphuret of mercury, whose weight was to that of the cyanuret of mercury, precisely as the weight of an atom of cinnabar, is to the weight of an atom of cyanuret of mercury.

This accordance with the results obtained by M. Gay-Lussac, satisfied me that the difference between my results, and those of Messrs. Porrett and Thomson cannot be attributed to my apparatus being less suited to the purpose.

I next repeated the same analysis with the ferruginous prussiate of barytes, previously dried at a strong heat. It gave traces of moisture in the tube which conducted the gases into the receiver, although its quantity was not great: 15.444 grs. of the salt employed gave 0.026 of a grain of water, precisely the quantity wanting, in the analysis mentioned above, to make the

oxygen of the water of crystallization amount to six times that of the protoxide of iron, presupposed in the salt, instead of five times and a half. The volume of the carbonic acid gas obtained in this experiment was again to that of the azote, as 3 : 2, and the whole of the carbonic acid obtained weighed 6.25 grains.

No conclusion can be drawn from these experiments before the quantity of carbonic acid retained by the base is determined. If the azote and the carbon in these salts be in the same proportion as in cyanogen, one-third of the quantity obtained, or one-fourth of the whole of the carbonic acid, is wanting. But if, on the contrary, the base remaining in the calcined mass be in the state of common carbonate, the volume of carbonic acid is to that of the azote as $2\frac{1}{2}$: 1.

(To be continued.)

ARTICLE XV.

ANALYSES OF BOOKS.

A Chemical and Medical Report of the Properties of the Mineral Waters of Buxton, Mallock, Tunbridge Wells, Harrogate, Bath, Cheltenham, Leamington, Malvern, and the Isle of Wight. By Charles Scudamore, M.D. Member of the Royal College of Physicians, &c. &c.

IN noticing this work, I shall, of course, confine my observations to the Chemical part of it, and I shall admit as proved, without making any inquiry, or expressing any doubt, that their medicinal qualities render these waters worthy of being drank; and there then can be no hesitation as to the importance of a perfect knowledge of their chemical constitution, whether it may or may not enable us to account for the beneficial effects which every day's experience would lead us to believe that they produce.

The analysis of the Buxton water appears to call for a few observations. I think the method employed to determine the presence of magnesia ambiguous, and the means used to ascertain the quantity of magnesian salt rather tedious. The plan adopted by Dr. Scudamore for ascertaining the presence of magnesia was that proposed by Dr. Wollaston, of first adding carbonate of ammonia, and then phosphate of soda to the water. Now I have found that the salt usually called carbonate of ammonia, and which is, in fact, a sesquicarbonate, always holds some carbonate of lime in solution; and this is particularly likely to occur when carbonate of lime exists in a mineral water, so

that the addition of phosphate of soda may throw down phosphate of lime, as well as the phosphate of ammonia and magnesia. Whether any difficulty occurred to Dr. Scudamore on this account in determining the quantity of magnesia, he does not mention, but I think it probable; for the method by which he determined the quantity of magnesia was that of decomposing the muriate obtained from the alcoholic solution, by means of carbonate of soda, then adding sulphuric acid to the carbonate of magnesia, suffering the sulphate of magnesia to crystallize by slow evaporation, and again decomposing the sulphate by carbonate of soda, and decomposing the carbonate of magnesia by heat, and then calculating the quantity of muriate of magnesia which it would yield.

From some late experiments I am induced to believe that lime water is not only the best test (with certain precautions), but also the most eligible substance for obtaining the magnesia from a mineral water.

Supposing no alkaline carbonate nor any alumina to exist in a water, and that the oxide of iron has been separated, lime water will discover an extremely minute portion of magnesia, when the saline contents of the water have been obtained by evaporation and redissolved in distilled water: Thus, I find that the twelfth part of a grain of magnesia, and even a much smaller quantity, may be readily shown to exist in a pint of water, and is easily precipitated from combination with an acid by lime water. The only inconvenience to be avoided, is the possibility that the excess of lime water used, may deposit carbonate, if the air be not carefully excluded during the filtering and washing of the precipitate.

It is, however, but due to Dr. Scudamore to state that I have found magnesia actually existing in this water, but which I did not believe to be the case until I read his work, and even when I had, I doubted as to the accuracy of his statement. My suspicions of the Doctor's inaccuracy were indeed strengthened by an experiment which he relates, and which I had tried with similar results, viz. that lime water occasions no precipitate in Buxton water which had been boiled: this experiment induced me to believe that the water contained no magnesia; but, on evaporating a portion to dryness, and redissolving the saline residuum in water, a precipitate of magnesia actually occurred. It will not be surprising that lime water did not indicate the magnesia in the water without concentration, when it is stated that the whole quantity which a gallon contains amounts to only about 0.16 of a grain. I have considered this part of the subject merely with a view of determining the best methods of detecting the presence of magnesia; for I will hazard the opinion, that in a medicinal point of view, the 0.16 of a grain of magnesia, however combined, cannot possess any very extensive influence when dissolved in a gallon of water. The

whole weight of the saline contents of a gallon of Buxton water amounts to fifteen grains; my experiments make it rather less; but I am inclined to think that Dr. Scudamore has stated the chemical analysis of this water with great accuracy.

In the analysis of the Tunbridge waters, I think Dr. Scudamore has been rather profuse in the application of the tests to indicate the presence of iron: thus, after observing an evident ochery deposit, the unequivocal action of tincture of galls, we have the confirming evidence of prussiate of potash, and sulphuretted hydrogen, which are again rendered doubly sure by the infusion of tea and coffee, whilst with cocoa or chocolate no action appears. Now with all deference, I submit, that these last observations might have been spared, as they serve merely to complicate the subject, and are devoid of any particular interest, for I have never heard of the application of Tunbridge water in preparing food.

The quantity of oxide of iron, obtained from Tunbridge water, Dr. Scudamore states to be 2.22 grains; it is not, perhaps, a matter of much importance, but I may observe that this quantity of iron is rather over-rated, for it was weighed in the state of peroxide, but ought to have been reduced to that of protoxide, in which iron always exists in mineral waters; this will make the quantity of oxide about 1.99 grain.

The analysis I shall next notice is that which Dr. Scudamore has given of the Bath water: this water I analyzed about seventeen years since; and, as well as every preceding and following analyst, I failed to ascertain the presence of magnesia, now shown to exist in the Bath water by Dr. Scudamore; my error arose from employing ammonia, on the supposition, which I did not then know to be erroneous, that this alkali would throw down minute quantities of magnesia. I have, by employing lime water in the mode already pointed out, ascertained the correctness of this author's statement. It appears that a pint of the water contains, however, only about 1.6 grain of muriate of magnesia.

In other respects there is no very material difference between Dr. S.'s analysis and mine; but I may observe that he committed a slight error in supposing me to have stated, that iron cannot be detected in the Bath water after it has cooled; what I have shown is, that if cooled without the presence of oxygen or atmospheric air, it retains its property of being acted upon by tincture of galls and showing the presence of iron; it is, indeed, upon this circumstance, that the peculiarity of the Bath water, as far as regards its chalybeate impregnation, depends.

The only remaining analysis which I shall notice is that of the Malvern water; some discussion has arisen between Dr. Philip, who formerly analyzed this water, and the author of the present work, as to whether this water contained any iron. Operating

in the usual way, I should certainly coincide with Dr. Scudamore, for it appears to me that neither tincture of galls, nor prussiate of potash, produces any appearance of iron. The question has, however, been set at rest by Mr. Children, who finds that, by evaporating the water, a very minute portion of iron is discoverable by prussiate of potash; but whether that quantity of iron, which cannot be detected by the common means, is likely to be useful in medicine is not for me to determine, but I may, perhaps, be allowed to question it. The quantity of iron contained in a gallon of Malvern water, according to Mr. Children's experiments, scarcely exceeds one seventieth of a grain.

Some difference of opinion also exists between Drs. Scudamore and Philip as to the existence of carbonate of soda in it. I evaporated a portion both from Holywell, and St. Anne's Well, to about 1-30 of its original volume, but I could not discover the slightest trace of any alkali. It appears, therefore, to me that Dr. Scudamore's statement is correct. In closing my observations upon this work, I wish to state that I cannot admit, with Dr. Scudamore, the position laid down by the late Dr. Murray, that when certain acids and alkaline bases are mixed together in solution, such salts are most likely to be formed as are most soluble in water. If I put together certain quantities of sulphate of soda, and muriate of lime, precipitation takes place, because sulphate of lime is formed; but is it likely that, as this theory supposes, the whole of the sulphate of lime formed is thrown down? Indeed, if I understand Dr. Murray's position, it amounts to this—that supposing I mix together sufficient quantities of muriate of lime and sulphate of soda, to form one hundred parts of sulphate of lime, that sulphate of lime will only be formed if there be not water enough to hold it in solution; but this would seem like attributing the property of insolubility to a compound before its formation.

It has, indeed, been attempted to strengthen this statement by arguing from the effects which certain mineral waters produce, or are supposed to produce, and which, according to common views, contain only sulphate of lime and muriate of soda; these waters are imagined to owe their good effects to containing not sulphate, but muriate of lime. It would, however, I think, require numerous experiments to prove that the same quantity of lime is more active as a medicine when combined with muriatic than sulphuric acid, when exhibited in equal quantities. I do not deny, but I question the fact; and if it should be proved, it may, as far as I am competent to give an opinion, be derived from the muriatic acid as from the lime.

I have not, in examining this work hitherto, expressed any decided opinion as to its merits; but I may add, that the chemical part of it appears to have been conducted with ability; and if the medical applications of the waters, have been as ably given as the chemical history, (of which I entertain no doubt,) the work must prove useful both to the chemist and physician.—*Ed.*

ARTICLE XVI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

March 8.—On the Length of the Second's Pendulum in different Latitudes, by Capt. Sabine.

March 15.—Observations on Naphthalin, by Dr. Kidd.

March 21.—On the Papyri of Herculaneum, by Sir H. Davy.

At the same meeting two other papers were read, viz. On the Aberration of compound Lenses, by Mr. Herschel; and On the Skeleton of the Dugong, by Sir E. Home.

An account of the contents of the above papers will be given in the next number.

GEOLOGICAL SOCIETY.

Jan. 14.—The Catalogue Raisonnée of the Collection of Swiss Rocks, sent by M. Lardi, was read.

The great valley comprehended between Mount Jura and the Alps, and which forms the lowest part of Switzerland, is composed partly of an alluvium, and partly of a sandstone and puddingstone, in Switzerland, called Nagelflue, which signifies literally nail-head-rock. The alluvium consists of insulated masses of primitive rocks, rolled pebbles, and debris, from the surrounding mountains; in this are also beds of clay.

The nagelflue may be divided into two parts; the molasse and the nagelflue, properly so called. The first is a sandstone used for building in that country; it is disposed in horizontal beds alternating with beds of marle, which sometimes contain marine shells and veins of fibrous gypsum: the lower part has beds of fetid bituminous limestone with freshwater shells, and thin layers of coal.

The nagelflue consists of a conglomerate of rounded pebbles of limestone united by a cement of the same nature; it forms a mass of 30 or 40 feet in thickness, and contains also beds of marle, and, occasionally, of coal. This appears to rest upon the calcareous rocks of the Alps.

The greatest part of Mount Jura consists of calcareous rock, which is usually compact, and of a yellowish colour; some of the beds are oolite. It incloses numerous marine shells. The beds of Jura dip on both sides of the chain. There is found here also a formation of clay ironstone, which extends nearly all the length of the Jura, and which supplies many foundries.

The Alpine limestone is considered as a transition formation; it is of a compact texture, of a greyish colour, and frequently contains nodules of flint. It rests on another limestone which alternates with slate.

The saliferous district of Bex has been well described by M.

Charpentier. It belongs to the transition formation, and rests at Lavey on primitive rocks. It is composed chiefly of a limestone, of which we distinguish two varieties; one is compact and oolitic, sometimes containing much silex: the other is an argillaceous limestone. It is in the last that the gypsum of the environs of Bex is situated. This gypsum is generally in the state of anhydrate, and presents several varieties. It is usually impregnated with muriate of soda, and sometimes contains beds of hydrous gypsum in the state of selenite; the fine crystals of which, from this place, are very celebrated. Native sulphur is also found. The beds subordinate to the gypsum are argillaceous limestone and slate clay.

Between Morcles and Lavey commences a transition formation, consisting of felspathic rocks, clay slate, and breccias, or puddingstone. This country is little known. Nearly the whole length of the Valais, a formation of clay slate extends: it contains also beds of limestone, of gypsum, and of quartz.

Above Oberswald are found rocks of gneiss, mica slate, clay slate, dolomite, and gypsum. These constitute the mountains of the Fourche of St. Gothard, and the superior part of the valley of the Tessin.

At Lago di Lugano, a blackish-brown porphyry is met with, consisting of hornstone with small crystals of white felspar.

ASTRONOMICAL SOCIETY OF LONDON.

At the commencement of the last year this interesting and important Society was founded. Although, at that time, we omitted the expression of our gratification on account of its formation, and even neglected to announce it, yet we fully participated with the cultivators of the science of astronomy in their enthusiastic expectations of the decided and powerful influence of this association upon its future progress. Indeed it had always appeared to us to be somewhat extraordinary, that while chemistry, geology, and several branches of natural history, were promoted and extended by associated bodies, a science, which, from the vastness of its objects, and the extent and difficulty of its observations and investigations, demands in the highest degree, the powerful aid of this concentration of intellect, should so long have been left to rely for its advancement on the labours of insulated and independent individuals. That this desideratum for astronomy will now be effectually supplied, cannot be doubted by those who have perused an exposition of the views and objects of the Society, in an address circulated prior to their first meeting, and a list of members affixed to the report presented by the Council to the first annual general meeting. Although the last, we boldly predict, that the Astronomical Society will not be the least, in the career of utility and fame.

Without entering too much into detail, we may, by an extract from the admirable address before alluded to, sufficiently display

the enlarged views and liberal feelings of the promoters of this design. "One of the first great steps towards an accurate knowledge of the construction of the heavens, is an acquaintance with the individual objects they present: in other words, the formation of a complete catalogue of stars, and of other bodies, upon a scale infinitely more extensive than any that has yet been undertaken; and that shall comprehend the most minute objects visible in good astronomical telescopes. To form such a catalogue, however, is an undertaking of such overwhelming labour as to defy the utmost exertions of individual industry. It is a task which, to be accomplished, must be divided among numbers; but so divided as to preserve a perfect unity of design, and prevent the loss of labour which must result from several observers working at once on the same region, while others are left unexamined. The intended foundation of an observatory at the southern extremity of Africa, under the auspices of the Admiralty, may serve to show the general sense entertained of the importance of this subject, and the necessity of giving every possible perfection to our catalogue of the fixed stars. Deeply impressed also with the importance of this task, and fully aware of its difficulty, the Astronomical Society might call upon the observers of Europe and of the world to lend their aid in its prosecution. Should similar institutions be formed in other countries, the Astronomical Society (rejecting all views but that of benefiting science) would be ready and desirous to divide at once the labour and the glory of this Herculean attempt, and to act in concert together in such manner as should be judged most conducive to the end in view." The following summary with which the address concludes, may be quoted in order to convey at once a distinct representation of the purposes of this Society. "The objects of the original members may be sufficiently gathered from what has been already said, and may be thus summed up in few words; viz. to encourage and promote their peculiar science by every means in their power, but especially—by collecting, reducing, and publishing, useful observations and tables—by setting on foot a minute and systematic examination of the heavens—by encouraging a general spirit of inquiry in practical astronomy—by establishing communications with foreign observers—by circulating notices of all remarkable phenomena about to happen, and of discoveries as they arise—by comparing the merits of different artists eminent in the construction of astronomical instruments—by proposing prizes for the improvement of particular departments, and bestowing medals and rewards for successful research in all;—and, finally, by acting as far as possible, in concert with every institution, both in England and abroad, whose objects have any thing in common with their own; but avoiding all interference with the objects and interests of established scientific bodies."

On the ninth of February, the first annual general meeting of

the Society was held; and the following gentlemen were elected officers for the year ensuing; viz.

President.—Sir W. Herschel, LLD. FRS.

Vice-Presidents.—H. T. Colebrooke, Esq. FRS. & LS.

S. Groombridge, Esq. FRS.

D: Moore, Esq. FRS. SA. & LS.

J. Pond, Esq. Astronomer Royal, FRS.

Treasurer.—Rev. W. Pearson, LLD. FRS.

Secretaries.—C. Babbage, Esq. MA. FRS. L & E.

F. Baily, Esq. FRS. & LS.

J. F. W. Herschel, Esq. MA. FRS. L & E.
(Foreign.)

Council.—Capt. T. Colby Roy. Eng. LLD. FRS. L & E.

Sir H. C. Englefield, Bart FRS. L & E. FSA. & LS.

Davies Gilbert, Esq. VPRS. and FLS.

B. Gompertz, Esq. FRS.

O. G. Gregory, LLD.

J. Rennie, Esq. FRS. L. & SE. A & LS.

J. South, Esq. FRS.

E. Troughton, Esq. FRS.

Trustees.—A. Baily, Esq.

D. Moore, Esq. FRS. SA. & LS.

C. Stokes, Esq. FRS. SA. & LS.

The Treasurer for the time being.

To this meeting an elegant and elaborate report was presented by the Council, which, having been adopted by the Society, was ordered to be printed. After congratulating the members on the success which has attended the first attempt to establish a Society for the promotion of so important a branch of science as astronomy, and stating that the efforts of its founders have been crowned with an accession of strength far beyond their most sanguine expectations, the Council proceeds to announce a plan of distributing medals, as an honorary reward, to such persons as may distinguish themselves by any material discovery, or improvement, in the science. The following extract will exhibit some of the subjects selected for the application of these stimulating rewards. "In the first place, it is proposed to bestow the medal for the discovery of any new planet, satellite, or comet; or for the rediscovery of any old comet, or of any stars that have disappeared. Considering also the great importance (both in a nautical and geographical point of view) of having accurate observations of the eclipses of Jupiter's satellites, and of occultations of stars by the moon, they think that the medal should be given for any considerable collection, not only of original observations of this kind, but also of well authenticated recorded observations, reduced to the mean time of the meridian of some well known observatory. Observations likewise on the posi-

tions of the fixed stars, tending either to the enlargement and perfection of our present catalogues, or to the more accurate determination of the variable ones in size, colour, or situation; as well as observations on double stars, tending in like manner not only to the enlargement and perfection of the present catalogues, but also to the determination of their angular distance and of their angle of position; together with observations on nebulæ,—appear proper subjects of such reward. To these may be added, observations on refraction, with a view to the more perfect theory of that phenomenon, particularly at low altitudes where irregularities take place, when little or no variation has taken place in the barometer or thermometer; observations on the tides, particularly in situations where the current is not influenced by any contiguous continent; observations tending to determine the true figure of the sun, or of the earth; and, in short, any observations which may be considered likely to advance and improve the science.”

Having mentioned several other subjects, such as the reduction of observations when made; the formation of various new tables, and the improvement of others; the comparison of the places of the planets; the examination of the recorded observations of preceding astronomers; and every improvement of instruments which may tend materially to advance the science, for which the Society's medals will be bestowed, the Council proceeds to recommend the proposal of the gold medal, and twenty guineas for the solution of the following question; a question requiring the synthetic application of the Newtonian doctrines, together with the highest refinements of modern analysis:

“For the best paper on the theory of the motions and perturbations of the satellites of Saturn. The investigation to be so conducted as to take expressly into consideration the influence of the rings, and the figure of the planet as modified by the attraction of the rings, on the motions of the satellites: to furnish formulæ adapted to the determination of the elements of their orbits, and the constant coefficients of their periodical and secular equations, from observation: likewise to point out the observations best adapted to lead to a knowledge of such determination. The papers to be sent to the Society on or before the first day of February, 1823.”

The Council, however, while thus taking advantage of the stimulus to inquiry which medals and prizes produce, have established, in the following impressive sentences, their claim to the possession of a full sense of the magnificence of their science, and the comparative insignificance of their rewards: “It may indeed appear extraordinary that no mention should yet have been made of the great desiderata of astronomy; those questions which have exercised the curiosity and employed the time and attention of astronomers, ever since the science has assumed its

present character—such as the parallax of the fixed stars, their proper motion; the motion or rest of our own system, and its connection with the rest of the universe. But these and many other points, are too obviously suggested by their importance to need any particular notice or encouragement. The man for whom discoveries of this class are reserved, soars far beyond any distinction which this Society can bestow: the applause of the human race attends his labours; and no additional stimulus can be offered to those by which he is impelled.”

In consequence of a communication from Captain Basil Hall, expressing his readiness to attend to any instructions on subjects wherein he might be of service to the science of astronomy in his intended voyage to the South Seas, the Council have delineated with great minuteness and ingenuity, their wishes in regard to his undertaking. The formation of an Astronomical Library, one of the objects of the Society, is then noticed; and the donations by the East India Company of many valuable papers on astronomical subjects, and of a series of observations made at the Observatory at Madras, are acknowledged. The alteration of several of the regulations; the appointment of Committees for various purposes; the notice of the discussion of a plan for examining the heavens in minute detail; and the statement of the constant accession and unqualified approbation of the continental astronomers, constitute the remaining topics of this report; which thus concludes: “On the whole, the Council cannot view this new impulse which appears to have been given to astronomy in all parts of the world, without anticipating the most beneficial results to the science. The establishment of several new Observatories on the continent of Europe (one of them above the sixtieth degree of north latitude) under the direction of men eminent in science, and vieing with each other in the most honourable branch of emulation—the rising efforts of our countrymen in the East Indies—the zeal of our brethren on the American continent—the foundation of a public Observatory at Cambridge, and another at the Cape of Good Hope (both so honourable to our own country)—must ensure the good wishes of every friend to science, and excite the admiration of every reflecting mind.”

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

An Analogical Inquiry into the probable Results of the Influence of Eruptive Eruptions in Hydrophobia, Tetanus, Non-Exanthematicus, and other Diseases incidental to the Human Body, illustrated by Cases.

Dr. Clark Abel is preparing a Translation from the German of Blumenbach's Elements of Natural History, comprehending considerable Additions.

Observations on some of the General Principles, and on the Particular Nature and Treatment, of the different Species of Inflammation; by J. H. James, Surgeon to the Devon and Exeter Hospitals, &c.

An Essay on Resuscitation, with a Representation and Description of an improved Apparatus. By T. J. Armiger, Surgeon.

Thomas Hare, FLS. &c. intends to publish a View of the Structure, Functions, and Disorders of the Stomach; and Alimentary Organs of the Human Body, with Physiological Observations and Remarks upon the Qualities and Effects of Food, and fermented Liquors.

JUST PUBLISHED.

Practical Observations on those Disorders of the Liver, and other Organs of Digestion, which produce the several Forms and Varieties of the Bilious Complaint. New and enlarged Edition. By Joseph Ayre, MD. 8s. 6d. bds.

Observations on Syphilis. By John Bacot. 8vo. 5s.

A Treatise on the Epidemic Cholera of India. By James Boyle. 8vo. 5s.

A Treatise on the Medical Powers of the Nitromuriatic Acid Bath in various Diseases, with Cases. By Walter Dunlop, Surgeon. 8vo. 2s.

Illustrations of British Ornithology. By P. J. Selby, Esq. Member of the Wernerian Natural History Society at Edinburgh. First Series, Land Birds, No. I. Elephant folio, 1l. 11s. 6d. coloured 5l. 5s.

Elementary Illustrations of the Celestial Mechanics of Laplace. 8vo. 10s. 6d. boards.

ARTICLE XVIII.

NEW PATENTS.

James Ferguson Cole, of Hans-place, St. Luke, Chelsea, for certain improvements in chronometers.—Jan. 27, 1821.

John Roger Arnold, of Chigwell, Essex, for a new or improved expansion balance for chronometers.—Jan. 27.

Alphonso Doxat, of Bishopsgate-street, for a new combination of mechanical powers, whereby the weight and muscular force of men may be employed to actuate machinery for raising water, or other purposes, in a more advantageous manner than has been hitherto practised, communicated to him by a certain foreigner residing abroad.—Jan. 27.

Phillips London, the younger, of Cannon-street, practical chemist, for a certain improvement in the application of heat to coppers and other utensils.—Feb. 3.

William Aldersey, of Homerton, for an improvement on steam-engines, and other machinery where the crank is used.—Feb. 3.

George Vizard, of Dursley, Gloucestershire, for a new process or method of dressing and polishing goods of woollen manufacture.—Feb. 3.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1821.	Wind.		BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a.m.
			Max.	Min.	Max.	Min.			
2d Mon.									
Feb. 1	S	W	30·36	30·21	52	44	—		63
2		W	30·32	30·21	50	31	—		80
3		W	30·25	30·05	48	39	—		77
4		W	30·62	30·03	44	25	—	—	70
5	N	W	30·74	30·62	39	24	—		67
6	S	W	30·76	30·69	42	27	—		60
7	S	W	30·69	30·61	45	27	—		68
8		S	30·61	30·25	49	21	—		55
9		Var.	30·30	30·11	45	29	—		73
10	N	E	30·37	30·30	46	28	—		73
11	N	E	30·37	30·30	45	27	—		61
12	N	E	30·33	30·28	40	32	—		80
13	N	E	30·33	30·28	39	30	55		66
14		Var.	30·38	30·28	34	31	—		59
15	N	E	30·53	30·38	35	21	—		74
16	N	E	30·53	30·41	38	26	—		71
17	S	E	30·41	30·24	33	26	—		80
18	N	W	30·33	30·20	39	27	—	—	67
19	N	W	30·33	30·30	40	22	—		66
20	N	W	30·31	30·17	40	30	—	·08	62
21	N	W	30·32	30·21	40	27	—		79
22	S	E	30·32	30·30	42	20	—		65
23	N	W	30·30	30·21	37	20	—		76
24	N	W	30·21	30·15	37	24	—		91
25	N	W	30·15	30·14	44	32	—	—	79
26		E	30·14	29·92	35	18	—		66
27	S	E	29·92	29·45	38	24	—	—	62
28	S	E	29·44	29·33	37	31	52	·23	71
			30·76	29·33	52	18	1·07	0·31	91—55

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Second Month.—1, 2. Fine. 3. Fine: cloudy. 4. Overcast. 5. Fine: hoar frost in the morning. 6. Hoar frost. 7. Hoar frost: very fine morning: lunar corona at night. 8. Hoar frost: fine. 9. Hoar frost: cloudy: fine. 10. Cloudy. 11. Cloudy: fine at intervals. 12—15. Cloudy. 16. Lunar corona. 17. Cloudy. 18. Fine: a shower about 10, p.m. 19. Fine. 20. Hoar frost: a shower in the evening. 21. *Cirrocumulus* and *Cirrostratus* streaked, like an agate. 22. Hoar frost. 23, 24. Hoar frost: foggy. 25. Cloudy. 26. Cloudy: bleak. 27. Very fine morning. 28. Snowy.

RESULTS.

Winds: NE, 6; E, 1; SE, 4; S, 1; SW, 3; W, 3; NW, 8; Var. 2.

Barometer: Mean height

For the month..... 30.272 inches.

For the lunar period, ending the 25th..... 30.336

For 14 days, ending the 5th (moon south) 30.404

For 13 days, ending the 18th (moon north) 30.395

Thermometer: Mean height

For the month..... 34.214°

For the lunar period..... 35.5

For 29 days, the sun in Aquarius. 36.741

Evaporation..... 1.07 in.

Rain. 0.31

Mean of hygrometer. 70°

The barometer, which has ranged very high most part of this winter, has twice risen as in the last and present month as probably to reach the upper extremity of the scale in many instruments, as it did in one of those which we observed.

*. The *Comet*, which is now visible, was well seen at Tottenham by my brother and other persons as early as the evening of the 23d; and on the 27th, the *Zodiacal Light* was also distinctly observed there.

Laboratory, Stratford, Third Month, 17, 1821.

R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1821.

ARTICLE I.

Observations on the Combinations of Azote and Oxygen.
By Thomas Thomson, MD. FRS.

SCARCELY any part of chemistry has been investigated with more industry than the various proportions in which oxygen and azote combine. This is partly to be ascribed to the beautiful simplicity which such combinations exhibit, partly to the apparent facility of experiments on the union of deutoxide of azote and common air, and partly to the notion entertained by Dr. Priestley and his contemporaries, that common air varies in the proportion of oxygen which it contains, that its state of salubrity depends upon this proportion, and that the degree of its goodness is easily determined by means of deutoxide of azote.

Chemists in general are now agreed about the number of compounds of azote and oxygen, which are capable of being formed; and, with the exception of Mr. Dalton, they are agreed likewise about the exact proportions in which they unite. Mr. Dalton, in a very elaborate paper, published in the ninth volume of the *Annals of Philosophy*, p. 186, has given us a great number of experiments, from which he draws the following conclusion: the five compounds of azote and oxygen, if we consider both of the constituents in the gaseous form, are composed of

Volumes.	Volumes.	
100 azote + 62 oxygen,	constituting	protoxide of azote
100 + 62 × 2 = 124		deutoxide of azote
100 + 62 × 3 = 186		hyponitrous acid
100 + 62 × 4 = 248		nitrous acid
100 + 62 × 5 = 310		nitric acid

Gay-Lussac, in a paper published in an early volume of the *Annales de Chimie et de Physique*, has given very strong reasons for concluding that the composition of these five compounds is as follows :

	Azote.	Oxygen.
Protoxide of azote.	100 volumes	+ 50 volumes
Deutoxide of azote	100	+ 100
Hyponitrous acid.....	100	+ 150
Nitrous acid	100	+ 200
Nitric acid	100	+ 250

As far as I know, these proportions of Gay-Lussac have been adopted by the whole chemical world, with the exception of Mr. Dalton. My object in this paper is to show that the present state of our knowledge leaves no doubt whatever that Gay-Lussac's proportions are accurate, and that Mr. Dalton has misled himself somehow or other.

1. I take it for granted that the specific gravity of oxygen and azotic gases are as follows :

Oxygen	1.1111
Azotic	0.9722

For the proofs of these specific gravities, I refer the reader to the *Annals of Philosophy*, xvi. 163.

2. I have demonstrated (*ibid.* p. 171) that the specific gravity of protoxide of azote is 1.5277.

Now we have 1 volume of azotic gas = 0.9722

+ volume of oxygen gas = 0.5555

1.5277

Thus when a volume of azotic gas is united to half a volume of oxygen, and the two condensed into one volume, a gas is formed which possesses exactly the specific gravity of protoxide of azote. Hence we are entitled to conclude, that protoxide of azote is a compound of one volume azotic + half a volume of oxygen gas condensed into one volume. That this is its true composition is obvious from the following experiment, which I have often made, and which very nearly agrees with the results given long ago by Davy in his Researches.

Mix together 100 volumes of protoxide of azote and 100 volumes of hydrogen gas. Pass an electrical spark through the mixture; detonation takes place, the whole hydrogen gas disappears, the residual gas measures exactly 100 volumes, and is pure azotic gas. Thus we see that 100 volumes of protoxide of azote contain exactly 100 volumes of azotic gas. The 100 volumes of hydrogen gas must have been converted into water, and for this conversion they must have united with 50 volumes

of oxygen gas; consequently 100 volumes of protoxide of azote contain exactly 50 volumes of oxygen gas.

The only difficulty attending this experiment is to procure protoxide of azote in a state of absolute purity. I have frequently had it so pure that the error in the results did not amount to so much as half per cent. This I consider as a demonstration that Dalton's proportion of oxygen, which he makes 62 volumes, is excessive. Were this the quantity, 124 volumes of hydrogen gas would be requisite instead of 100 volumes; and after burning a mixture of 100 volumes of protoxide of azote and 100 volumes of hydrogen, a portion of protoxide of azote (amounting to rather more than 20 volumes) should remain undecomposed, or 12 volumes of oxygen gas should be found in the residual gas, which, instead of 100 volumes, would amount to 112 volumes.

If Mr. Dalton will consider these facts, he will, I think, see the necessity of admitting that protoxide of azote is a compound of 100 volumes azotic + 50 volumes oxygen gas condensed into 100 volumes.

2. That deutoxide of azote is a compound of 100 volumes azotic + 100 volumes oxygen gas united together, and constituting 200 volumes, will not admit of doubt, if we attend to its specific gravity, which I have shown to be 1.04166 (*Annals of Philosophy*, xvi. 172.) Now this is exactly the mean of the specific gravities of oxygen and azotic gases; for

Oxygen	= 1.1111
Azotic	= 0.9722
	<hr/>
	2)2.0833
	<hr/>
Mean	= 1.04166

I have not myself made any experiments to determine the quantity of oxygen in deutoxide of azote by combustion; but the evidence adduced by Gay-Lussac in the second volume of the *Memoires d'Arcueil*, that this gas is composed of equal volumes of oxygen and azotic gas, is so conclusive as to leave no doubts whatever on my mind of its truth.

3. I have attempted to verify Gay-Lussac's experiments, in which he made one volume of oxygen gas unite with four volumes of deutoxide of azote; but though I have returned to the subject more than once at different periods of the year, I have never been so fortunate as to obtain the same results with that very ingenious philosopher. I have, however, succeeded in hitting upon a method by which this combination can be accomplished at all times with the greatest ease. Indeed I have since found that this method is not new. It was practised by Mr. Cavendish as long ago as 1783, and is minutely described

by him in his well-known paper, entitled "Of a new Eudiometer," (Phil. Trans. 1783, p. 106). It is, therefore, rather singular, that this method, which is easy and exact, seems to be quite unknown both to Mr. Dalton and M. Gay-Lussac. The method is this : Put into a small glass vessel open below 100 volumes of deutoxide of azote. Into a small cylindrical glass vessel having a small brass stop cock at its top, and a brass plate (with a small hole in it) fixed to it by grinding below, put 100 volumes of common air. Sink the cylindrical vessel under water, and place the vessel containing the deutoxide of azote over it, taking care that its mouth is under the surface of the water. Things being in this situation, open the stop cock. The common air will make its way slowly, and bubble by bubble, into the deutoxide of azote. Agitate the vessel containing the deutoxide of azote the whole time that the common air is entering into it. If you measure the residual gas after the process is at an end, it will amount at an average to 96 or 97 volumes ; so that the quantity of gas that disappears when the experiment is made in this way amounts to 103 or 104 volumes.

Now 100 volumes of common air contain 21 volumes of oxygen gas ; so that 82 or 83 volumes of the gas which have disappeared are deutoxide of azote and 21 volumes oxygen gas ; but $21 : 83 :: 100 : 395\cdot2$.

In this mode of experimenting then, 100 volumes of oxygen gas unite to $395\cdot2$ volumes of deutoxide of azote. Now $395\cdot2$ is only about one per cent. less than 400. I conceive, therefore, that there can be no doubt when the experiment is made in this way, that 100 volumes of oxygen gas really unite with 400 volumes of deutoxide of azote.

Deutoxide of azote consisting of equal volumes of azotic and oxygen gases united without undergoing any condensation, it is obvious that 400 volumes of it must be composed of

200 volumes oxygen
200 volumes azotic gas

Therefore, when 100 oxygen unite with 400 deutoxide of azote, the compound formed is in reality composed of 200 volumes azotic and 300 volumes oxygen ; or, which is the same thing, of 100 volumes azotic + 150 volumes of oxygen gas. Now this is the compound called *hyponitrous acid*. I have never attempted to collect this acid in order to examine its properties, the great quantity of water with which it was diluted in all my experiments precluding the possibility of obtaining it. But as this mode of experimenting gives uniform results, I see no reason to doubt that such a substance as hyponitrous acid actually exists.

4. I have never found any difficulty in obtaining a compound of 100 volumes oxygen and 200 volumes of deutoxide of azote. My method of proceeding is this : I introduce 100 volumes of common air into a cylindrical glass tube, the internal diameter

of which is 0.9 inch. This tube is shut at one end and open at the other, and being filled with water is placed inverted on the shelf of the water trough. A hundred volumes of deutoxide of azote are let up to the common air. The mixture becomes yellow, and diminishes rapidly in volume. I allow the tube to stand untouched till the gas has become clear, and till it ceases sensibly to diminish in bulk. I then introduce the residual gas into a graduated tube, and measure its volume. The average residue, when the experiment is made in this way, is 137 volumes. The following table exhibits the residual volume in six successive experiments, the original volume of the two gases being 200.

Volumes.

136

137

137

138

136

137

Mean 136.8

Thus it appears that when the experiment is made in this way, the gas which disappears, and which of course must have been converted into an acid and absorbed by the water, amounts to 63 volumes. Now 21 of these volumes must have been the oxygen contained in the 100 volumes of common air, and the remaining 42 volumes must have been deutoxide of azote; but 42 is just double 21. Thus we see that when the experiment is made in this way, one volume of oxygen combines with two volumes of deutoxide of azote. This is the same thing as 100 volumes of oxygen and 200 volumes of deutoxide of azote; but 200 volumes of deutoxide of azote are composed of

100 volumes oxygen,

100 volumes azotic gas,

consequently the acid formed in this case is a compound of 100 volumes of azotic and 200 volumes of oxygen gas. It is, therefore, the acid known by the name of *nitrous acid*. Dulong has shown that this is the acid which is obtained when nitrate of lead is exposed to heat in a retort; while the receiver is surrounded with a mixture of snow and salt. It is an acid of a pale-yellow colour, and seems to undergo decomposition when united to the bases. From Dulong's experiments, it appears to contain no water. Indeed nitrate of lead, when properly dried, is free from water.

5. I have not yet hit upon a method of uniting 100 volumes of oxygen with 133 volumes of deutoxide of azote. Davy first showed, that if we unite these two gases in these proportions,

the resulting compound will be nitric acid. I have tried the experiments, which he describes, but find the results too variable to place any reliance on them. But it is not difficult to demonstrate that nitric acid is in reality a compound of 100 volumes azotic and 250 volumes of oxygen gas.

In the *Annals of Philosophy*, xvi. 334, I have shown by a simple, but decisive experiment, that the atomic weight of nitric acid is 6.75. The preceding observations leave no doubt that the composition of protoxide of azote, deutoxide of azote, hyponitrous acid, and nitrous acid, is as follows :

	Azote.	Oxygen.
Protoxide of azote	100 volumes	+ 50 volumes
Deutoxide of azote	100	+ 100 or 50×2
Hyponitrous acid.	100	+ 150 or 50×3
Nitrous acid.	100	+ 200 or 50×4

We cannot avoid concluding, from observing these proportions, that these different substances are composed as follows :

	Azote.	Oxygen.
Protoxide of azote	1 atom	+ 1 atom
Deutoxide of azote	1	+ 2
Hyponitrous acid.	1	+ 3
Nitrous acid.	1	+ 4

Consequently an atom of azote is represented by a volume of azotic gas and an atom of oxygen by half a volume of oxygen gas ; but the specific gravity of azotic gas is 0.9722, and that of oxygen 1.1111, the half of which is 0.5555 ; therefore, the weight of an atom of oxygen is to that of an atom of azote as 0.5555 to 0.9722 ; but $5555 : 9722 :: 1 : 1.75$; consequently, if an atom of oxygen be represented by 1, an atom of azote will weigh 1.75. Substituting these weights for the atoms in the preceding table, we obtain the atomic weights of these different bodies as follows :

Protoxide of azote	2.75
Deutoxide of azote.	3.75
Hyponitrous acid	4.75
Nitrous acid.	5.75

If to 5.75 we add another atom of oxygen, we obtain 6.75. Now this is the weight of an atom of nitric acid. It cannot be doubted, therefore, that nitric acid is a compound of five atoms of oxygen and one atom of azote. But as an atom of azote represents a volume, while an atom of oxygen represents half a volume, it is evident that nitric acid must be a compound of

Azote.

Oxygen.

100 volumes + 250 volumes.

Now this is the very constitution pointed out by Davy and by Gay-Lussac.

6. About the year 1806, Mr. Dalton published a set of experiments to determine the proportion of the several gases in the atmosphere.* In this paper he remarks, that if 36 volumes of pure deutoxide of azote be introduced into a glass tube about three-tenths of an inch wide, and 100 volumes of common air be let up into it, after a few minutes the whole will be reduced to 79 or 80 volumes, and will exhibit no signs of either oxygen, or deutoxide of azote. In this case, 21 volumes of oxygen have united with 36 volumes of deutoxide of azote. If the same experiment be made in a wide vessel, a common tumbler, for instance, and if we employ 72 volumes of deutoxide of azote and 100 volumes of common air, the residue will be as before, 79 or 80 volumes. Thus 21 volumes of oxygen unite with 36 volumes of deutoxide of azote in a narrow tube, and with $36 \times 2 = 72$ volumes in a wide vessel. This is equivalent to

Oxygen.

Deutoxide of azote.

100 volumes + 171.429 volumes

100

+ 342.858

The diminution of volume in a narrow tube he finds so constant that he recommends this mode of experimenting as a good method of detecting the volume of oxygen in a given quantity of gas. Take a given volume of it, and let up into it a given volume of deutoxide of azote. Note the diminution of volume; $\frac{7}{19}$ ths of this diminution is the oxygen required; so that, according to this rule, we have only to multiply the diminution of volume by 0.3684; the product is the volume of oxygen required.

I have made a great many trials to verify these conclusions of Mr. Dalton. Indeed I adopted his mode of determining the proportion of oxygen in mixed gases by means of deutoxide of azote as soon as I became acquainted with it; but the want of coincidence between different trials, though made in precisely the same way, led me at last to doubt its precision, and to make a set of experiments in order to investigate what really takes place. I found, in the first place, that the results did not vary sensibly, whether we employed glass tubes of the bore 0.3, 0.4, or 0.5 inch. My mode of proceeding was to put 100 volumes of common air into a graduated tube, and to let up into it 100 volumes of deutoxide of azote. After the diminution of volume was at an end, I noted the volume of residual gas. The following table exhibits the volume of residual gas in six successive experiments made in this way in a tube of 0.5 inch in diameter:

* Phil. Mag. xxiii. 351.

Volumes.

138
140
144
142
144
142

The mean of these trials gives a residue of 141·6 volumes. The smallest residue was 138, and the greatest 144.

The mean volume of gas which disappeared in this case was 58·4. Now of this, 21 volumes were oxygen gas, the remaining 37·4 volumes must have been deutoxide of azote; so that the mean of these experiments gives us 21 volumes of oxygen uniting with 37·4 volumes of deutoxide of azote. This is only a very little greater than 36 volumes, the quantity assigned by Mr. Dalton. The extremes in the experiments are :

Volumes.

Volumes.

21 oxygen + 35 deutoxide of azote
21 + 41

These variations are so great that I was induced to abandon Dalton's method altogether. I find that a tube 0·9 inch in diameter gives much more correct results. When we employ it, the 21 volumes of oxygen just unite with 42 volumes of deutoxide of azote; so that the oxygen is obtained by dividing the diminution of bulk by 3. It is obvious that 36 volumes of deutoxide of azote is not the minimum quantity with which 21 volumes of oxygen gas are capable of uniting. The minimum, instead of 36, is in reality 28 volumes. I have obtained a diminution not exceeding 51 volumes, when I employed very narrow tubes; but the process is disagreeable, and not nearly so accurate as when we use tubes with a diameter of 0·9 inch.

When we mix common air and deutoxide of azote in a common tumbler over water, the results are pretty uniform. The following table exhibits the volume of residual gas when 100 volumes of deutoxide of azote were let up into 100 volumes of common air in a tumbler about three inches in diameter :

119
119
118
118
118
118

Mean 118·3

In these experiments, the mean diminution of bulk was 81½

volumes. Of these 21 were oxygen; so that 21 volumes of oxygen had united with 60·6 volumes of deutoxide of azote. This differs materially from 72 volumes which Mr. Dalton states as the maximum of deutoxide of azote which unites with 21 volumes of oxygen. My experiments were all made without agitating the vessels, which no doubt diminishes the portion of deutoxide of azote which disappears when agitation is used. My results approach very nearly to one volume oxygen, and three volumes deutoxide of azote. Such a compound would consist of

1·5 volumes azotic,
2·5 volumes oxygen.

This is equivalent to

1 volume azotic,
 $1\frac{1}{3}$ volume of oxygen,

which is the same as 1 atom azote + $3\frac{1}{3}$ atoms oxygen. This is obviously no definite compound, though it approaches nearest to hyponitrous acid.

A very great number of experiments which I have made upon these combinations during the course of the last 15 years leave no doubt whatever on my mind that both Mr. Dalton's minimum and maximum of deutoxide of azote are inaccurate, and that in reality 1 volume of oxygen may be made to combine with $1\frac{1}{3}$, 2, and 4 volumes of deutoxide of azote, producing nitric acid, nitrous acid, and hyponitrous acid, respectively. The two gases can combine in all the intermediate proportions between these. Hence the great variety of results, and the apparently capricious nature of the experiments, that have for so many years attracted the attention of the chemical world.

ARTICLE II.

A Memoir on some new Modifications of Galvanic Apparatus, with Observations in Support of his Theory of Galvanism.
By R. Hare, MD. Professor of Chemistry in the University of Pennsylvania. Communicated by the Author. (With a Plate.)

I HAD observed that the ignition produced by one or two galvanic pairs attained its highest intensity, almost as soon as they were covered by the acid used to excite them, and ceased soon afterwards; although the action of the acid should have increased during the interim. I had also remarked in using an apparatus of 300 pairs of small plates, that a platina wire, No. 16, placed in the circuit, was fused in consequence of a construction which enabled me to plunge them all nearly at the same time. It was, therefore, conceived, that the maximum of effect

380 *Dr. Hare's new Galvanic Apparatus, Theory, &c.* [MAY, in voltaic apparatus of extensive series had never been attained. The plates are generally arranged in distinct troughs, rarely containing more than 20 pairs. Those of the great apparatus of the Royal Institution, employed by Sir H. Davy, had only 10 pairs in each. There were 100 such to be successively placed in the acid, and the whole connected ere the poles could act. Consequently the effect which arises immediately after immersion would be lost in the troughs first arranged, before it could be produced in the last; and no effort appears to have been made to take advantage of this transient accumulation of power, either in using that magnificent combination, or in any other of which I have read. In order to observe the consequence of simultaneous immersion with a series sufficiently numerous to test the correctness of my expectations, a galvanic apparatus of 80 concentric coils of copper and zinc was so suspended by a beam and levers as that they might be made to descend into, or rise out of, the acid in an instant. The zinc sheets were about nine inches by six, the copper fourteen by six; more of this metal being necessary, as in every coil it was made to commence within the zinc, and completely to surround it without. The sheets were coiled so as not to leave between them an interstice wider than a quarter of an inch. Each coil is in diameter about two inches and a half, so that all may descend freely into 80 glass jars two inches and three-quarters diameter inside, and eight inches high, duly stationed to receive them.*

My apparatus being thus arranged, two small lead pipes were severally soldered to each pole, and a piece of charcoal about a quarter of an inch thick, and an inch and a half long, tapering a little at each extremity, had these severally inserted into the hollow ends of the pipes. The jars being furnished with diluted acid, and the coils suddenly lowered into them, no vestige of the charcoal could be seen. It was ignited so intensely, that those portions of the pipes by which it had been embraced were destroyed. In order to avoid a useless and tiresome repetition, I will here state that the coils were only kept in the acid while the action at the poles was at a maximum in the experiment just mentioned; and in others, which I am about to describe, unless where the decomposition produced by water is spoken of, or the sensation excited in the hands. I designate the apparatus with which I performed them as the galvanic deflagrator, on account of its superior power, in proportion to its size, in causing deflagration; and as, in the form last adopted, it differs from the voltaic pile in the omission of one of the elements heretofore deemed necessary to its construction.

Desirous of seeing the effect of the simultaneous immersion of my series upon water, the pipes soldered to the poles were introduced into a vessel containing that fluid. No extraordinary

* See Plate VII.

effect was perceived, until they were very near, when a vivid flash was observed, and happening to touch almost at the same time, they were found fused and incorporated at the place of contact. I next soldered to each pipe a brass cylinder of about five-tenths of an inch bore. These cylinders were made to receive the tapering extremities of a piece of charcoal about two inches long, so as to complete the circuit. The submersion of the coils caused the most vivid ignition in the coal. It was instantaneously and entirely on fire. A piece of platina of about a quarter of an inch diameter in connexion with one pole, was instantly fused at the end on being brought in contact with some mercury communicating with the other. When two cylinders of charcoal, having hemispherical terminations, were fitted into the brass cylinders and brought nearly into contact, a most vivid ignition took place, and continued after they were removed about a half or three quarters of an inch apart, the interval rivalling the sun in brilliancy. The igneous fluid appeared to proceed from the positive side. The charcoal in the cylinder soldered to the latter, would be intensely ignited throughout, when the piece connected with the negative pole was ignited more towards the extremity approaching the positive. The most intense action seems to arise from placing a platina wire of about the eighth of an inch diameter, in connexion with the positive pole, and bringing it in contact with, and afterwards removing it a small distance apart from, a piece of charcoal (fresh from the fire) affixed to the other pole.

As points are pre-eminently capable of carrying off (without being injured) a current of the electrical fluid, and very ill qualified to conduct caloric; while, by facilitating radiation, charcoal favours the separation of caloric from the electricity which does not radiate; this result seems consistent with my hypothesis, that the fluid as extricated by Volta's pile is a compound of caloric and electricity; * but not with the other hypothesis,

* According to the theory here alluded to, the galvanic fluid owes its properties to caloric and electricity, the former predominating in proportion to the size of the pairs, the latter in proportion to the number, being in both cases excited by a powerful acid. Hence in batteries which combine both qualifications sufficiently, as in all those intervening between Children's large pairs of two feet eight inches by six feet, and the 2000 four-inch pairs of the Royal Institution, the phenomena indicate the presence of both fluids. In De Luc's column, where the size of the pairs is insignificant, and the energy of interposed agents feeble, we see electricity evolved without any appreciable quantity of caloric. In the calorimeter where we have size only, the number being the lowest possible, we are scarcely able to detect the presence of electricity.

When the fluid contains enough electricity to give a projectile power adequate to pass through a small space in the air, or through charcoal, which impedes or arrests the caloric, and favours its propensity to radiate, this principal heat is evolved. This accounts for the evolution of intense heat under those circumstances which rarifies the air, so that the length of the jet from one pole to the other may be extended after its commencement. Hence the portions of the circuit nearest to the intervening charcoal, or heated space, are alone injured; and even non-conducting bodies, as quartz, introduced into it are fused, and hence a very large wire may be melted by the fluid, received through a small wire imperceptibly affected.

See Silliman's Journal, No. 6, vol. i.; Thomson's *Annals*, Sept. 1810; Tilloch's *Philosophical Magazine*, Oct. 1819.

which supposes it to be electricity alone. The finest needle is competent to discharge the product of the most powerful machines without detriment, if received gradually as generated by them. Platina points, as small as those which were melted like wax in my experiments, are used as tips to lightening rods without injury, unless in sudden discharges produced under peculiar circumstances.*

The following experiment I conceive to be very unfavourable to the idea that galvanic ignition arises from a current of electricity.

A cylinder of lead, of about a quarter of an inch diameter, and about two inches long, was reduced to the thickness of a common brass pin for about three-quarters of an inch. When one end was connected with one pole of the apparatus, the other remained suspended by this filament; yet it was instantaneously fused by contact with the other pole. As all the caloric fluid which acted upon the suspended knob must have passed through the filament by which it hung, the fusion could not have resulted from a pure electrical current, which would have dispersed the filament ere a mass 50 times larger had been perceptibly affected. According to my theory, caloric is not separated from the electricity until circumstances very much favour a disunion, as on the passage of the compound fluid through charcoal, the air, or a vacuum. In operating with the deflagrator, I have found a brass knob of about five-tenths of an inch in diameter, to burn on the superficies only; where alone, according to my view, caloric is separated so as to act on the mass. Having, as mentioned in the memoir on my theory of galvanism, found that four galvanic surfaces acted well in one recipient, I was tempted, by means of the 80 coils, to extend that construction. It occurred to me that attempts of this kind had failed from using only one copper for each zinc plate. The zinc had always been permitted to react towards the negative as well as the positive pole. My coils being surrounded by copper, it seemed probable, that if electro-caloric were, as I had suggested, carried forward by circulation arising from galvanic polarity, this might act within the interior of the coils, yet not be exerted between one coil and another.

I had accordingly a trough constructed with a partition along the middle, so as to receive 40 coils on one side, and a like number on the other. This apparatus, when in operation, excited a sensation scarcely tolerable in the backs of the hands. Interposed charcoal was not ignited as easily as before, but a most intense ignition took place on bringing a metallic point connected with one pole of the series into contact with a piece of charcoal fastened to the other. It did not take place, however, so speedily as when glasses were used; but soon after the ignition was effected, it became even more powerful than before. A cylinder

* See Adams's Electricity, on Points.

of platina nearly a quarter of an inch in diameter, tapering a little at the end, was fused and burned so as to sparkle to a considerable distance around, and fall in drops. A ball of brass of about half an inch diameter was seen to burn on its surface with a green flame. Tin foil, or tinsel, rolled up into large coils of about three-quarters of an inch thick, were rapidly destroyed, as was a wire of platina of No. 16. Platina wires in connexion with the poles were brought into contact with sulphuric acid; there was an appearance of lively ignition, but strongest on the positive side. Excepting in its power of permeating charcoal, the galvanic fluid seemed to be extricated with as much force as when each coil was in a distinct glass. Apprehending that the partition in the trough did not sufficiently insulate the poles from each other, as they were but a few inches apart, moisture or moistened wood intervening, I had two troughs made; each to hold 40 pairs, and took care that there should be a dry space about four inches broad between them. They were first filled with pure river water, there being no saline nor acid matter to influence the plates, unless the very minute quantity which might have remained on them from former immersions. Yet the sensation produced by them on the backs of my hands was painful, and a lively scintillation took place when the poles were approximated. Dutch gold leaf was not sensibly burned, though water was found decomposable by wires properly affixed. No effect was produced on potash, the heat being inadequate to fuse it.

A mixture of nitre and sulphuric acid was next added to the water in the troughs, afterwards charcoal from the fire was vividly ignited, and when attached to the positive pole a steel wire was interposed between it and the other pole, the most vivid ignition which I ever saw was induced. I should deem it imprudent to repeat the experiment without glasses, as my eyes, though unusually strong, were affected for 48 hours afterwards. If the intensity of the light did not produce an optical deception by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion. That charcoal should be thus softened, without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise, the air could have no access; first, because of the excessive rarefaction, and in the next place, as I suspect, on account of the volatilization of the carbon forming about it a circumambient atmosphere. This last mentioned impression arose from observing that, when the experiment was performed in vacuo, there was a lively scintillation, as if the carbon in an aeriform state acted as a supporter of combustion on the metal.

A wire of platina (No. 16) was fused into a globule on being connected with the positive pole, and brought into contact with

a piece of pure hydrate of potash, situated on a silver tray in connexion with the other pole. The potash became red-hot, and was deflagrated rapidly with a flame having the rosy hue of potassuretted hydrogen.

The great apparatus of the Royal Institution, in *projectile power*, was from six to eight times more potent than mine. It produced a discharge between charcoal points when removed about four inches apart, whereas mine will not produce a jet at more than three-fourths of an inch. But that was a series of 2000 pairs; mine is only about a twenty-fifth part as large.

A steel wire of about one-tenth of an inch in diameter affixed to the negative pole was passed up through the axis of an open necked inverted bell glass filled with water. A platina wire, No. 16, attached to a positive pole, being passed down to the steel wire, both were fused together, and cooling, could not be separated by manual force. Immediately after this incorporation of their extremities, the platina wire became incandescent for a space of some inches above the surface of the water.

A piece of silvered paper, about two inches square, was folded up, the metallic surface outward, and fastened into vices affixed to the poles. Into each vice a wire was screwed at the same time. The fluid generated by the apparatus was not perceptibly conveyed by the silvered paper, as it did not prevent the wires severally attached to the poles from decomposing water, or producing ignition by contact.

In my memoir on my theory of galvanism, I suggested that the decomposition of water, which Wollaston effected by mechanical electricity, might not be the effect of divellent attraction like those excited by the poles of a voltaic pile, but of a mechanical concussion, as when wires are dispersed by the discharge of an electrical battery. In support of that opinion, I will now observe, that he could not prevent hydrogen and oxygen from being extricated at each wire, instead of hydrogen being given off only at one, and oxygen at the other, as is invariably the case when the voltaic pile is employed. That learned and ingenious philosopher, in concluding his account of this celebrated experiment, says, "but in fact the resemblance is not complete, for in every way in which I have tried it, I observed each wire gave out both oxygen and hydrogen gas, instead of their being formed separately as by the electric pile."

Is it not reasonable to suppose that an electrical shock may dissipate any body into its elementary atoms, whether simple or compound, so that no two particles would be left together which can be separated by physical means.

Looking over Singer's *Electricity*, a recent and most able modern publication, I find that in the explosion of brass wire by an electrical battery, the copper and zinc actually separated. He says, page 186, "Brass wire is sometimes *decomposed* by the charge; the copper and zinc of which it is formed being

separated from each other, and appearing in their distinct metallic colours." In the next page of the same work, I find that the oxides of mercury and tin are reduced by electrical discharges. "Introduce," says the author, "some oxide of tin into a glass tube, so that when the tube is laid horizontal, the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lay between them. Pass several strong charges in succession through the tube, replacing the oxide in its situation, should it be dispersed. If the charges are sufficiently powerful, a part of the tube will soon be stained with metallic tin which has been revived by the action of transmitted electricity." It cannot be alleged that in such decompositions the divellent polar attractions are exercised like those which characterize the action of wire proceeding from the poles of a voltaic apparatus. The particles were dispersed from, instead of being attracted to the wires, by which the influence was conveyed among them. This being undeniable, it can hardly be advanced that we are to have one mode of explaining the separation of the elements of brass by an electrical discharge, another of explaining the separation of the elements of water by the same agent. One rationale when oxygen is liberated from tin, and another when liberated by like means from hydrogen. In the experiment in which copper was precipitated by the same philosopher at the negative pole, we are not informed whether the oxygen and acid in union with it were attracted to the other, and the changes produced in litmus are mentioned not as simultaneous, but successive. The violet and red rays of the spectrum have an opposite chemical influence in some degree like that of voltaic poles, but this has not led to the conclusion that the cause of galvanism and light is the same. Besides admitting that the feeble results obtained by Wollaston and Van Marum with electrical machines are perfectly analogous to those obtained by the galvanic pile, ere it can become an objection to my hypothesis, it ought first to be shown that the union between caloric and electricity, which I suppose productive of galvanic phenomena, cannot be produced by that very process. If they combine to form the galvanic fluid when extricated by ordinary galvanic action, they must have an affinity for each other. As I have suggested in my memoir, when electricity enters the pores of a metal, it may unite with its caloric. In Wollaston's experiments, being constrained to enter the metal, it may combine with enough of its caloric to produce, when emitted, results slightly approaching to those of a fluid in which caloric exists in greater proportion.

But once more I demand why, if mechanical electricity be too intense to produce galvanic phenomena, should it be ren-

336 *Dr. Hare's new Galvanic Apparatus, Theory, &c.* [MAY, dered more capable of producing them by being still more concentrated.

If the one be generated more copiously, the other more intensely, the first will move in a large stream slowly, the last in a small stream rapidly. Yet, by narrowing the channel of the latter, Wollaston is supposed to render it more like the former; that is, produces a resemblance by increasing the supposed source of dissimilarity.

It has been imagined that the beneficial effect of his contrivance arises from the production of a continued stream, instead of a succession of sparks; but if a continued stream were the only desideratum, a point placed near the conductor of a powerful machine would afford this requisite, as the whole product may in such cases be conveyed by a sewing needle in a stream perfectly continuous. As yet no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the processes of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity is accounted for, according to my hypothesis, by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluid from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attractions which it may exercise, operates almost without restraint. This is fully exemplified in my galvanic deflagrator, where 80 pairs are suspended in two recipients, 40 successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation* when excited only by fresh river water. I have already observed that the reason why galvanic apparatus, composed of pairs consisting each of one copper and one zinc plate, have not acted well without insulation† was because electro-caloric could retrocede in the negative as well as advance in the positive direction. I will now add that, independently of the greater effect produced by the simultaneous immersion of my 80 coils, their power is improved by the proximity of the surfaces, which are only about

* I do not say shock, as it is more like the permanent impression of a pointed wire; and when an acid is used, of a hot one.

† That is, with the same mass of conducting fluid, in contact with all the surfaces, instead of being divided into different portions, each restricted in its action to one copper and one zinc plate.

three-sixteenths of an inch asunder ; so that the circulation may go on more rapidly.

Pursuant to the doctrine, which supposes the same quantity of electricity, varying in intensity in the ratio of the number of pairs to the quantity of surface, to be the sole agent in galvanic ignition, the electrical fluid as evolved by Sir H. Davy's great pile must have been nearly two thousand times more intense than as evolved by a single pair, yet it gave sparks at no greater distance than the thirtieth or fortieth of an inch. The intensity of the fluid must be at least as much greater in one instance than in another, as the sparks produced by it are longer. A fine electrical plate machine, of 32 inches diameter, will give sparks at 10 inches. Of course the intensity of the fluid which it emits must be 300 times greater than that emitted by 2000 pairs. The intensity produced by a single pair must be 2000 times less than that produced by the great pile ; and, of course, 600,000 times less than that produced by a good electrical plate of 32 inches. Yet a single pair, of about a square foot in area, will certainly deflagrate more wire than a like extent of coated surface charged by such a plate. According to Singer, it requires about 160 square inches of coated glass, to destroy watch pendulum wire ; a larger wire may be burned off by a galvanic battery of a foot square. But agreeably to the hypothesis in dispute, it compensates by quantity for the want of intensity. Hence the quantity of fluid in the pair is 600,000 times greater, while its intensity is 600,000 times less ; and *vice versa* of the coated surface. Is not this absurd ? What does intensity mean as applied to a fluid ? Is it not expressed by the ratio of quantity to space ? If there be twice as much electricity within one cubic inch, as within another, is there not twice the intensity ? But the one acts suddenly, it may be said ; the other slowly. But whence this difference ? They may both have exactly the same surface to exist in. The same zinc and copper plates may be used for coatings first, and a galvanic pair afterwards. Let it be said, as it may in truth, that the charge is, in the one case, attached to the glass superficies ; in the other, exists in the pores of the metal. But why does it avoid these pores in one case, and reside in them in the other ? What else resides in the pores of the metal which may be forced out by percussion ? Is it not caloric ? Possibly, unless under constraint, or circumstances favourable to a union between this principle and electricity, the latter cannot enter the metallic pores beyond a certain degree of saturation ; and hence an electrical charge does not reside in the metallic coatings of a Leyden phial, though it fuses the wire which forms a circuit between them.

It is admitted that the action of the galvanic fluid is upon, or between, atoms ; while mechanical electricity, when uncoerced, acts only upon masses. This difference has not been explained unless by my hypothesis, in which caloric, of which the influence

338 *Dr. Hare's new Galvanic Apparatus, Theory, &c.* [MAY, is only exerted between atoms, is supposed to be a principal agent in galvanism. Nor has any other reason been given that water, which dissipates pure electricity, should cause the galvanic fluid to accumulate. From the prodigious effect which moist air, or a moist surface, has in paralysing the most efficient machines, I am led to suppose, that the conducting power of moisture so situated is greater than that of water under its surface. The power of this fluid to conduct mechanical electricity is unfairly contrasted with that of a metal, when the former is enclosed in a glass tube, the latter bare.

According to Singer, the electrical accumulation is as great when water is used as when more powerful menstrua are employed; but the power of ignition is wanting until these are resorted to. De Luc showed, by his ingenious dissections of the pile, that electricity might be produced *without*, or *with* chemical power. The rationale of these differences never has been given, unless by my theory, which supposes caloric to be present in the one case, but not in the other. The electric column was the fruit of De Luc's sagacious inquiries, and afforded a beautiful and incontrovertible support to the objections he made to the idea, that the galvanic fluid is pure electricity, when extricated by the voltaic pile in its usual form. It showed that a pile really producing pure electricity is devoid of the chemical power of galvanism.

We are informed by Sir H. Davy that, when charcoal points in connection with the poles of the magnificent apparatus with which he operated, were first brought nearly into contact, and then withdrawn four inches apart, there was a heated arch formed between them, in which such non-conducting substances as quartz were fused. I believe it impossible to fuse electrics by mechanical electricity. If opposing its passage, they may be broken, and if conductors near them be ignited, they may be acted on by those ignited conductors as if otherwise heated; but I will venture to predict, that the slightest glass fibre will not enter into fusion by being placed in a current from the largest machine, or electrical battery.

I am induced to believe that we must consider light, as well as heat, an ingredient in the galvanic fluid; and think it possible, that, being necessary to vitality in animals, as well as vegetables, the electric fluid may be the vehicle of its distribution.

I will take this opportunity of stating, that the heat evolved by one galvanic pair has been found, by the experiments which I instituted, to increase in quantity, but to diminish in intensity, as the size of the surfaces may be enlarged. A pair containing about 50 square feet of each metal will not fuse platina, nor deflagrate iron, however small may be the wire employed; for the heat produced in metallic wires is not improved by a reduction in their size beyond a certain point. Yet the metals above-

mentioned are easily fused or deflagrated by smaller pairs, which would have no perceptible influence on masses that might be sensibly ignited by larger pairs. These characteristics were fully demonstrated, not only by my own apparatus, but by those constructed by Messrs. Wetherill and Peale, and which were larger, but less capable of exciting intense ignition. Mr. Peale's apparatus contained nearly 70 square feet, Mr. Wetherill's nearly 100, in the form of concentric coils, yet neither could produce a heat above redness on the smallest wires. At my suggestion, Mr. Peale separated the two surfaces in his coils into four alternating, constituting two galvanic pairs into one recipient. Iron wire was then easily burned, and platina fused by it. These facts, together with the incapacity of the calorific fluid extricated by the calorimeter to permeate charcoal, next to metals, the best electrical conductor, must sanction the position I assigned to it as being in the opposite extreme from the columns of De Luc and Zamboni. For, as in these, the phenomena are such as are characteristic of pure electricity, so in one very large galvanic pair, they almost exclusively demonstrate the agency of pure caloric.

P. S. Since writing the above, I have endeavoured in every mode which I could devise to ignite charcoal by electricity. Exposed to the discharge of a powerful battery in pieces tapering to a point, in a glass tube, in thin strips, and in powder, by means of the glass usually employed for inflaming ether, it was either uninfluenced, or merely dispersed, without the smallest symptom of ignition, or even of increased warmth. Yet fulminating mercury was flashed by the discharge, under the same circumstances as those in which the powdered charcoal had been subjected to it. The result, therefore, was such as might be expected from a "*mechanical concussion*." Pointed wires were covered with spermaceti, and exposed to a current from a fine plate machine of 32 inches diameter; yet no sign of fusion appeared. Nor was a differential thermometer filled with ether, according to Dr. Howard's sagacious plan, affected sensibly, though the warmth of a finger applied to the bulb caused the fluid in the stem to move nearly a foot.

I mentioned in the memoir, p. 332, that when a knob of lead suspended by a filament to one of the poles of my deflagrator was made to touch the other pole of the same instrument, the knob was fused, the filament uninjured. I find the reverse is the case, when a knob suspended by a filament is made the medium of discharging an electrical battery. The filament is destroyed, the knob remains unchanged. It must be evident, therefore, that galvanic and electrical ignition are extremely discordant in their characteristics.

It is also mentioned in the memoir that a piece of silvere

340 *Mr. Herapath on the Causes, Laws, and principal* [MAY, paper, two inches square, proved inadequate to discharge my galvanic apparatus of coils, yet at a distance 70 times greater, a strip of the same paper, one-third of an inch wide, and 20 inches long, caused an instantaneous discharge of the electrical battery.

ARTICLE III.

A Mathematical Inquiry into the Causes, Laws, and principal Phenomena of Heat, Gases, Gravitation, &c. By John Herapath, Esq. (In a Letter to D. Gilbert, Esq. MP. VPRS. &c.)

(Continued from p. 293.)

OF THE LAWS OF GASEOUS BODIES.

Definitions.

Def. 1.—The homogeneity of a gas is the perfect equality of its atoms, or particles, throughout, in quantity of matter. It is of no consequence whether the atoms are similar in figure or not; their figures might be very different, provided, however, the quantity of solid matter in each is the same.

Def. 2.—Density is the quantity of matter in a given space, when the atoms, or particles, whatever be their relative magnitudes, are uniformly disposed in the medium.

Cor. 1.—The mean density of any body is, therefore, proportional to the whole quantity of matter, divided by the whole bulk, or magnitude of the body.

Cor. 2.—In a homogeneous body, the density is equal to the mass of an atom drawn into the number contained in a given space.

Def. 3.—Numeratom is a term I have employed to express the number of atoms, or particles, distributed throughout a given space, without respect to the density or homogeneity of the body.

Cor.—Hence the numeratom of a homogeneous body is proportional to the density directly, and the mass of a particle, or atom, inversely.

This conclusion might also have been drawn from *Cor. 2* of the preceding definition.

Def. 4.—The elasticity of a gas is the force with which it endeavours to expand itself, or with which it resists compression; and is estimated by the amount of its action against similar and equal portions of the containing bodies.

GENERAL CONSIDERATIONS ON FORCE AND THE CONSTITUTION OF GASEOUS BODIES.

Force is the cause of any change, or is that power, by which the change is produced. If, therefore, a body be perfectly free

to yield to any change of state, and none take place, it follows, that the body is not acted on by any force; or if it be, it must be by an accumulation of opposing forces, which, in the aggregate, destroy one another. Mathematicians usually estimate force by the effect it produces, or would produce, in a given time, and consider the intensity of the force as proportional to the effect. Thus, if one of two forces produce twice the effect of the other, it is considered to be doubly as powerful; if three times the effect, trebly; and so on. But this is rather estimating the effects than the forces; since it is possible for the same primitive force, by successive actions, to produce very unequal effects in equal times.

Forces have likewise been distinguished into two kinds, pressive and impulsive, or those which act by pressure, and those which act by impulsion. The former generate changes in bodies by a continued unceasing action, from the beginning to the end of the stroke, or fit, of action; and, consequently, always consume time. The latter act either by an instantaneous impulse, or by a succession of those impulses; but each impulse, individually considered, occupies not the smallest portion of time. It is an action that in one moment might be said not to have come into existence; and in the next to have ended. This is the kind of force we have now to consider.

If we take single impulses, it is plain that the forces and the changes they produce are proportional, under the same circumstances of action. But if a succession of impulses takes place, the effects are no longer proportional to the forces, but to the collected actions of the forces in equal times. When, therefore, the forces are equal, the effects are proportional to the numbers of their respective repetitions; and when those forces are unequal, but uniformly intense in their actions, the effects are in the compound ratio of those numbers and the forces. It is hence manifestly possible for a weak force, by a greater number of successive actions, to produce effects equal to those of a stronger by a less number of actions; and if the impulses of the two forces be at regular intervals, and yet the intervals of the slower be not sensible to observation, the effects will always have the same ratio, whatever be the length of time in which they are compared.

These impulses may also be opposed to a pressive force, and effect precisely the same things. For let us suppose a hard body to be acted on from a state of quiescence by a continued force, such, for instance, as what we commonly conceive gravity to be, urging it in any direction; then, after it has been impelled forward for any length of time by this force, and has acquired a certain momentum, let it be met by another hard body, moving uniformly with an equal momentum in the opposite direction; and by our laws of collision, it will receive such a change in its motion by the contact as will give it an equal and opposite

momentum in the path it has already described. It will, therefore, being acted on still by the soliciting force, retrace this path, until, having ascended to the point whence it set out, it has lost all its velocity, and begins to descend again as before. As soon as it has arrived again to the place where it before was met by the other body, let it receive a second equal shock, and again it will begin to ascend; and so on as long as we please. Thus it might be continually kept oscillating between these two points by impulses properly regulated. And if the distance between the points be diminished, the number of oscillations will be increased, and the intervals between the shocks diminished, and the body consequently approach nearer to a state of apparent rest.

This is how things will happen when the affected body comes in contact with only one other body, and receives the strokes in the direction of the track of its centre of gravity. But the same would evidently take place, if, instead of one body striking it in this identical manner, it were struck by several in different parts of its surface, either all at once, or in any order; the aggregate direction and intensity of whose strokes, however, should be similar to those of the single body under similar circumstances. Then might the body acted on by the force remain in a state of rest, if not absolute, at least so nearly so, as to differ from it insensibly. Now the tendency of this body to move in the direction in which it is solicited is precisely the same thing as mathematicians understand by the force, which they call pressure; and, therefore, the opposition of the other smaller bodies is also equivalent; and, as to effect, the same as this force of pressure. And because the value of this pressure might be increased or diminished; by increasing or diminishing the soliciting force, or the pressing body; so also might the value of the resisting force of the smaller bodies be increased or diminished, by increasing or diminishing their momentum, the number of their collisions, or the number of the bodies.

From these considerations it follows, that if a number of small bodies be inclosed in any hollow body, and be continually impinging on one another, and on the sides of the enclosing body; and if the motions of the bodies be conserved by an equivalent action in the sides of the containing body, then will these small bodies compose a medium, whose elastic force will be like that of our air and other gaseous bodies; for if the bodies be exceedingly small, the medium might, like any aeriform body, be compressed into a very small space; and yet, if it had no other tendency than what would arise from the internal collision of its atoms, it would, if left to itself, extend to the occupation of a space of almost indefinite greatness. And its temperature remaining the same, its elasticity would also be greater when occupying a less, and less when occupying a greater space; for in a condensed state the number of atoms striking against a

given portion of the containing vessel must be augmented; and the space in which the atoms have to move being less, their returns, or periods, must be shorter; and the number of them, in a given time, consequently greater, on both of which accounts the elasticity is greater, the greater the condensation. Besides, when other things are the same, the elastic force augments with an augmentation of temperature, and diminishes with a diminution; for an increase of temperature, according to our theory, must necessarily be attended with an increase of velocity; and, therefore, with an increase in the number of collisions. But these things will be more accurately treated of presently.

Whether all pressive forces be not the same as this gaseous action, is a question we do not at present intend to consider. It might come under our cognizance hereafter, when we shall have had more opportunities of collating our principles with experiments. In the mean time it is sufficient to anticipate that our inquiries, whatever be the possibility of the existence of another kind of pressive force, lead us to conclude that nature operates by no other.

It might be asked by what means is it, if the parts of the gases are absolutely hard, that they are reflected back into the medium from the sides of the containing vessel? This question is easily answered, if we allow heat to consist in an intestine motion of the parts of the bodies; for then the parts of a solid, of equal temperature with a gas, must have the same quantity, though they have not the same freedom of motion as the parts of a gas have; and hence the parts of the gas will have the same reflection from the sides of the vessel that they have from one another.

PROP. VI.

The figure of the vessel, in which a gas is contained, has no influence on the elastic force of the gas, such a constitution of things being granted as we have supposed.

For since the particles of the gas receive their reflection from the superficial particles of the containing body; and since these particles have not only their intestine motion, but likewise their figure and arrangement, independent of the superficial figure of the containing body, it follows, that the direction of their action on the particles of the gas is also independent of the figure of the body in which they are enclosed. But other things being alike, this action is equal to the contrary action of the particles of the gas, which constitutes its elastic force; therefore, its elastic force is not affected by the figure of the body in which it is contained.

Cor.—Hence we gather that in any bodies of equal capacity and of the same temperature, the same, or an equal portion of the same gas, would be equally elastic; for the motion of the particles being independent of the figure of the containing body must depend entirely upon the temperature of that body; and

344 *Mr. Herapath on the Causes, Laws, and principal* [MAY, this being the same, the motion is the same. But the motion, quantity of gas, and capacity, being the same, the elasticity must be the same.

Scholium.

This theorem and its cor. agree with the opinion of philosophers respecting the elasticity of gaseous bodies, though I am not aware that they have ever been made the subject of direct experiment. Indeed it seems to have been taken for granted that wherever the temperatures, spaces occupied, gases, and quantities of gas, were the same, the elasticities were the same; but it would be worth while to be more certain of it. An experiment to settle this point, taking every circumstance into consideration, would, perhaps, require as much care and skill as almost any of those that have been made on gases.

PROP. VII.

If a given portion of a fluid gas, composed of particles mutually impinging on one another and the sides of the containing body, in the manner that has been described, has its temperature the same; and if the particles be indefinitely small, its elastic force, under different compressions, is reciprocally proportional to the space it occupies.

Let us suppose that equal portions of the same gas be enclosed in two vessels of unequal capacity. Then, by the last Prop. it is immaterial whether these vessels be of the same or of different figures; the difference of figures would have no influence upon the ratio of the elasticities; but, for the sake of simplicity, we will suppose the two figures similar. Now because the only change that is supposed to take place is in the space which the gas occupies, the motions and collisions of a particle in the one will be similar to those of a corresponding particle in the other; and the temperature, that is, in this case, the velocity being the same in each, the numbers of revolutions that two corresponding particles in the two media make in a given time must be inversely proportional to the paths the particles describe; that is, these paths being alike and described with equal velocities in the inverse subtriplicate ratio of the spaces occupied by the equal portions of gases. But because the elasticity of a gas is proportional to the action of its particles against a given portion of the surface of the containing body, the ratio of the elastic forces, arising from the repeated actions of equal numbers of corresponding particles in the two media, will likewise, their velocities being the same in both media, be inversely as the subtriplicate of the spaces occupied. And if we conceive the two gases to be divided into strata, parallel to the sides of the bodies on which the elastic forces are measured, and of one, two, or any number of particles thick, it is manifest, since the motions of the particles are alike in each medium, that if the elasticity

in one medium arises from the action of the particles of the first stratum alone, so it does also in the other medium; and if it arises from the action of the particles of the two first, three first, or n first strata in one medium, the same holds true with the elasticity in the other medium. But the number of particles of any one stratum that strike against a given portion of the containing surface of one medium, is to the number of particles of the corresponding stratum that strike against an equal and similar portion of the other medium, in the duplisubtriplicate ratio of the numeratons directly; that is, in the duplisubtriplicate ratio of the spaces occupied by equal portions of the gases inversely. Therefore as the whole elastic forces of these corresponding strata are in a ratio compounded of the ratios of the numbers of particles that strike against equal portions of the sides of the containing bodies, and of the numbers of returns which they make to the sides in a given time, that ratio must be equal to one compounded of the inverse duplisubtriplicate and of the inverse subtriplicate ratios of the spaces occupied by the two gases; it must, therefore, be equal to the simple inverse ratio of the spaces occupied. And since the same number of strata affects the elasticity of the one gas as of the other; and since the inverse ratio of the spaces is the ratio of the elastic force of any two corresponding strata, it is consequently the ratio of the united elastic forces of all the strata that affect the elasticity; and is, therefore, the ratio of the elastic forces of the two gases.

Cor.—Because the numeratons are reciprocally proportional to the spaces occupied, it follows that the elasticities are, under equal temperatures, directly as the numeratons.

Scholium.

We have in the two preceding theorems and their corollaries supposed the atoms, or particles, to be perfectly hard; but the same consequences would follow if they were either perfectly or imperfectly elastic, and the containing vessel either elastic or hard. For the temperature being invariable, the intensity of the collisions, and consequently of the reflections, would remain the same in a rare as in a denser medium. The law, therefore, that the elasticities and compressions are proportional, under equal temperatures, is true not only in permanent airs or gases, but in all kinds of vapours, which is conformable to experience.

PROP. VIII.

The same things remaining, the elasticity of a gas under a variable temperature and compression, is proportional to its numeraton and the square of its temperature conjointly; or the elasticity varies as the square of the temperature directly, and the simple of the space inversely.

If we first suppose in two portions of the same gas the numeratons to be equal, the elasticities of these portions will have the

346 *Mr. Herapath on the Causes, Laws, and principal* [MAY, same ratio as the elasticities arising from the actions of corresponding particles in the two media; for the change of temperature does not alter the manner in which the corresponding particles act in the media, but only the intensity of action. This being the case, the elasticities due to the actions of corresponding particles are to one another as their momenta and the number of their revolutions or returns in a given time; that is, as their temperatures and velocities. But the masses of the corresponding particles being the same, the velocities are as the temperatures; therefore, the elasticities due to corresponding particles, and consequently the elasticities of the media, are as the squares of the temperatures. And by the cor. to the preceding prop. the temperatures being the same, the elasticities are as the numeratons. Whence, if neither the temperatures, nor the numeratons are the same, the elasticities are in a ratio compounded of the ratio of the numeratons, and that of the squares of the temperatures, or, which is the same, in a ratio compounded of the inverse ratio of the volumes and the duplicate direct of the temperatures.

Cor. 1.—Hence the elasticities are also in a ratio which is equal to that compounded of the simple ratio of the numeratons, and the duplicate of the velocities.

Cor. 2.—And hence also the elasticities are in a ratio compounded of these three simple ratios; namely, the ratio of the numeratons, the ratio of the temperatures, and the ratio of the velocities.

PROP. IX.

The spaces occupied by equal portions of the same gas, under equal elasticities, are directly proportional to the squares of the temperatures.

For by the preceding proposition, the elasticity varies as the numeratom and the square of the temperature conjointly; therefore, the elasticity being constant, the numeratom is inversely as the square of the temperatures. But the quantity of gas being the same, the space occupied is reciprocally as the numeratom; consequently the space, or volume, is directly as the square of the temperature.

Cor. 1.—Because this is true of any gas, it follows that equal volumes of any gases whatever, under equal pressures and temperatures, will be equally augmented by equal augmentations of temperature.

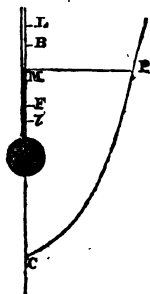
Cor. 2.—Or more generally, if the elasticities of any two gases have an invariable ratio, and if their temperatures also have an invariable ratio, their volumes will have an invariable ratio.

Cor. 3.—It has been found by MM. Dalton and Gay-Lussac, and lately confirmed by the further experiments of MM. Dulong and Petit, that the volume of a given portion of gas at the temperature of water freezing is to its volume under an equal pres-

sure at the temperature of water boiling as 8 to 11. Therefore, granting the truth of our principles, the temperature of water freezing is to that of water boiling in the subduplicate ratio of 8 to 11; that is, nearly in the ratio of 6 to 7; or, more nearly, in that of 100 to 117, or that of 579 to 679.

Cor. 4.—Because the temperatures are in the same gas as the velocities, the spaces occupied at equal elasticities are as the square of the velocities.

Cor. 5.—It has been demonstrated by the experiments of philosophers, that the volumes of mercury and air, under the same pressure, go on *pari passu* within certain limits; namely, from about -36 of centigrade to nearly 150 . Therefore, let AB be a common mercurial thermometer, of which F is the freezing and B the boiling point of water; and let the tube, or a line by the side of it, be continued to the point C , so that $BC : FC :: 11 : 8$; and let CB be made the axis of a parabola, whose vertex is C ; then will any semiordinate MP , between the limits l (-36) and L (nearly 150), drawn from the surface M of the mercury, represent the temperature of the body in which the thermometer is plunged; and CM the volume, due to the expansion of air under the same temperature and a uniform pressure.



From these circumstances it appears, that the measures which have been taken by MM. Dulong and Petit, in their late memoir on the communication of heat, are not proportional to the temperatures, as they imagine, but to the squares of the temperatures.

Cor. 6.—Since, if the temperatures are the same, the volumes are inversely proportional to the elasticities, it follows that the volumes being the same, the squares of the temperatures are as the elasticities. Hence, therefore, the same results would be obtained by measuring the temperatures by the elasticities, under an invariable volume, as by the volumes under an invariable compression, which is conformable to what MM. Dulong and Petit say they have found from experiment. This same inference I had drawn from theory several years before Dulong and Petit had made, or at least had published their experiments; and upon this principle I had contrived a thermometer by means of hydrogen and mercury, the description of which I have by me.

Scholium.

In the preceding theorems and their corollaries, we have not considered whether the media be homogeneous or not, or whether their particles be similar or dissimilar. The theorems are totally independent of all considerations of homogeneity or similarity, and

348 *Mr. Herapath on the Causes, Laws, and principal* [MAY, are, therefore, equally applicable to all gases, simple, or compound, supposing, however, their particles to be indefinitely small, or their diameters to have no sensible proportion to the lines they describe.

One of the longest known laws of gaseous bodies is, that of the volume being reciprocally proportional to the elasticity. This law is demonstrated in prop. 7, and is one that I have been careful to establish by a clear and explicit proof, as well on account of its own importance, as of its being the foundation of most of my other deductions on the properties of gaseous bodies. I have, however, shown, in the scholium to prop. 7, that the same law would result from the supposition of the particles being either perfectly or imperfectly elastic. But though this is true in the present instance, it is not so universally. For if we suppose a medium to be composed of elastic particles, and to be kept in a gaseous state by the actions of its particles on one another, and on the particles of the containing body; and if we also suppose the temperature to be equal to the momenta which these gaseous particles impress on the particles of the other bodies, then we find that, in order to preserve the temperature of the containing body, the gaseous particles must have such motions as will repel the particles of the containing body, with momenta equal to the momenta which they had previous to the contact. But to do this, the particles of the gas, if they and the particles of the containing body are unequal, will have different motions before and after the collision, which is evidently absurd. And if the gas should be transferred into a different body, whose particles are larger or smaller than the particles of the other body, the temperature and elasticity of the gas will be changed, though both the capacity and temperature of the two containing bodies should be the same. Besides, the temperatures of the gas and of the containing bodies would never be the same, if their particles were unequal. Nor could a gas, constituted of elastic particles, follow the laws of either of the other propositions.

If any method were known of experimentally determining the ratio of the temperatures of two bodies, we might easily devise ways enough of putting our theory to the test of observation; but since this is not the case, independent of theorems drawn from our principles, we are obliged to search for such consequences as are not under the controul of the ratio, or of the quantity of temperature. The inference drawn in the first cor. to prop. 9 is precisely of this kind, and presents us with a law that is easily examined by experiment. Indeed this law was discovered several years ago by MM. Dalton and Gay-Lussac, and has been established by many experiments. It is one of the most important laws, relative to the expansion and contraction of gaseous bodies, that has yet been discovered; and affords not

only a striking instance of the coincidence of our theory of the constitution of gases with phænomena, but also a fine corroboration of our theory of collision.

In cor. 2, I have generalized this law, by which means we have an opportunity of comparing the theory with phænomena on a more extensive and varied scale. For if we suppress the ratio of the temperatures, by making it the ratio of equality, this cor. will, in a variety of ways, become comparable with observation. By taking portions of any two gases, and measuring their elasticities and volumes at any common temperature, we ought by this cor. to find, that if we raise or diminish the temperatures equally, and make the ratios of the first and second volumes equal, the ratios of the first and second elasticities ought to be equal. Again, if the two temperatures be equal, and we any how change the temperature, elasticity, and volume of one of the gases; and if we make the second elasticities and volumes to hold respectively the same ratios as the first; then ought the second temperature of the one gas to be equal to the second temperature of the other.

PROP. X.

If the ratio of the elasticities of any two gases be that of e to 1, the ratio of their volumes that of v to 1, and of their temperatures that of t to 1; and if the elasticities, volumes, and temperatures of these gases be any how varied, so that the ratios of the second elasticities, volumes, and temperatures, be respectively those of e_1 to 1, v_1 to 1, and t_1 to 1 then will the ratio of e_1 to 1 be equal to that of $e t_1^2 v$ to $t^2 v_1$.

For let us call V, V the first volumes; E, E the first elasticities; and T, T the first temperatures of the gases; and let V_1, V_1 in like manner denote their second volumes: E_1, E_1 their second elasticities; and T_1, T_1 their second temperatures. Then by hypothesis, we have

$$\begin{aligned} V : V :: v : 1 \text{ and } V_1 : V_1 :: v_1 : 1 \\ E : E :: e : 1 \quad E_1 : E_1 :: e_1 : 1 \\ T : T :: t : 1 \quad T_1 : T_1 :: t_1 : 1 \end{aligned}$$

But by prop. 8,

$$\begin{aligned} E_1 : E :: T_1^2 V : T^2 V_1, \text{ and} \\ E : E_1 :: T^2 V_1 : T_1^2 V, \text{ and} \\ E : E :: e : 1. \end{aligned}$$

Therefore compounding these ratios, we shall get $E_1 : E$ or $e_1 : 1 :: T_1^2 T^2 V V_1$, $e : T^2 T_1^2 V_1 V$; and consequently $e_1 : 1 :: e t_1^2 v : t^2 v_1$.

Cor. 1.—From this theorem it appears, that if the ratio of the first temperatures be a ratio of equality; and if the same be the case with the ratio of the second temperatures, no matter what be the ratio of the first and second temperatures; and if the

350 *Mr. Herapath on the Causes, Laws, and principal* [MAY, same hold good with the ratios of the first and second volumes, then will the ratio of the first elasticities be equal to that of the second. That is, the volumes and temperatures of two gases always preserving ratios of equality, however much these volumes and temperatures may vary, the elasticities will maintain a given ratio. This is precisely what follows from cor. 2 to the preceding prop.

Cor. 2.—Supposing the same things to hold good, respecting the temperatures, the theorem becomes $v_1 : 1 :: \left\{ \begin{matrix} e : 1 \\ 1 : e_1 \\ v : 1 \end{matrix} \right\}$; that

is, the ratio of the second volumes is equal to the ratio compounded of these three ratios; namely, the direct ratio of the first elasticities, the inverse of the second, and the direct of the first volumes.

N. B. The same things will also hold good in both these corollaries, if the two ratios of temperatures, instead of being ratios of equality, are equal.

Cor. 3.—In the construction of Mr. Leslie's differential thermometer, it is supposed, when the two balls are of equal temperatures, that the elasticities and volumes are also equal. Therefore in this case, e , t , and v , are each unity, and $t_1 : 1 :: \sqrt{\left\{ \begin{matrix} v_1 : 1 \\ e_1 : 1 \end{matrix} \right\}}$

which, because the alteration in volume is but trifling, is nearly equal to $\sqrt{e_1 : 1}$. Hence, if we know the ratios of the second volumes and elasticities, we also know the ratio of the second temperatures; or if we know only the ratio of the second elasticities, this ratio of the second temperatures is nearly ascertained; and, therefore, one of the second temperatures being known, the other, and the difference of the two, become known. But the ratio of the volumes is easily ascertained, by admeasurements previous to the making of the instrument; and the difference in the elastic forces is determined by the difference in the altitudes of the fluid in the two legs, from which and by keeping one of the legs at a certain temperature, and by having measured the elastic force of the gas at that temperature, before the finishing of the instrument, we shall be enabled to obtain the difference and ratio of the two elasticities, and thence the temperature of the gas in the other leg.

I have for want of time but just mentioned this ingenious and useful little instrument, whose theory is contained in the preceding prop. but he that wishes to be fully acquainted with its merits and the variety of purposes to which it is applicable, may consult Mr. Leslie's Essay on Heat and Moisture, published a few years since.

Scholium.

It has in cor. 5 to prop. 8 been remarked that the augmentations of volume in mercury and any gas have so nearly a given

proportion, between certain temperatures, that scarcely any difference can be observed. With this fact, therefore, it will be no difficult matter to examine the preceding theorem by direct experiment, in a more general way than we have yet supposed; for all the other ratios might be easily determined by methods already known, and the temperatures might be measured between the limits alluded to by a common mercurial thermometer, either in the way that is pointed out in cor. 5, prop. 8, or by dividing the thermometer into equal parts, letting the freezing point stand at 80° and the boiling at 110° ; in which case the divisions would indicate the squares of the temperatures. Thus all the ratios might be obtained by experiment independently of each other; consequently, by combining any of them according to the theory, we shall be able to see whether the result agrees with that given by experiment.

Having now demonstrated, as they flow from our principles, some of the chief properties of gaseous bodies, after the manner of mathematicians, and in a way that I hope will satisfy the Royal Society, both of the legitimacy and simplicity of those principles, I shall throw some other things, which it appears necessary to add, into a general scholium.

(To be continued.)

ARTICLE IV.

A Memoir on the Physiology of the Egg, read before the Linnean Society of London, on March 21, 1809; an Abstract of which is published in the Society's Transactions. By John Ayrton Paris, MD. &c. &c.

[Having accidentally seen Dr. Paris's paper on the Physiology of the Egg, which had been printed, about 10 years since, for private circulation, it appeared to me to contain so much curious matter that I was desirous of inserting it in the *Annals*. Dr. Paris has not only kindly consented to my request, but has added several new and interesting particulars to his original memoir.—*Edit.*]

“At certe Natura, si fieri potuisset, maxime optasset suum opificium esse immortale; quod cum per materiam non liceret, subsidium quod potuit ipsi ad immortalitatem est fabricata, nam mirabilem quandam rationem invenit, quomodo in demortui animalis locum, novum aliud sufficiat.”—*Galen de Usu Partium.*

THE extensive range which the *Ovipari* form in the scale of animated existence renders the organization and developement of the egg a subject of great and general interest to the naturalist; while the hope of ascending to the source of vitality, by

contemplating life at a period when the number and complication of its functions are the least, becomes an irresistible inducement to the physiologist to pursue the investigation : hence we find that the philosophers of every age and nation have devoted much time and labour to this inquiry. Unfortunately, however, for science, the influence of chemical powers in the scheme of animal life has only of late been investigated in reference to the problem ; but many beneficial results have already attended this new train of research, and the most exhausted topics of natural history have assumed novel and very unexpected aspects. The author, therefore, of the present memoir may reasonably hope to escape the censure which must otherwise have awaited the adventurer who could presume to beat the field that has before been so ably and diligently explored by the united labours of Fabricius ab Aqua pendente, Harvey, Malpighi, Spallanzani, Hunter, and other physiologists. A powerful phalanx of philosophers maintain, with much plausibility, that the egg* is the universal womb of nature, and that *oviparous* differ only from *viviparous* animals by the latter breaking their ovular bondage† before they escape from the parent. Concerning the truth of this opinion, which is comprehended in the popular aphorism, “*Omnia ex Ovo*,” or with regard to the success with which the eloquent Count de Buffon has levelled his shafts against the partisans of this theory, I shall leave abler disputants to decide. The observations which I am prepared to submit to your notice do not involve the truth of either theory, but are connected only with those animals that are *oviparous*, in the common acceptance of the term ; that is, *who deposit a germ to be developed by causes totally independent of parental influence*.

Among the countless multitudes and varieties of animals, a very small proportion only are *viviparous*, or produce living offspring : thus the immense tribes of birds, fishes, amphibious animals, and insects, with comparatively few exceptions,‡ pro-

* Egg.—The word *Ovum* seems to be derived from the Greek word *Ola*, *solidum*, because it produces only one offspring ; thus Fabricius “*quia singularia pariat, non enim veluti ceteri uteri intra se plures gignunt fœtus*.” That each egg should include but one embryo seems to be a general law of nature, but not without its exceptions ; a singular species of egg was found by Mr. Folks, President of the Royal Society, in the mud of a rivulet, which equalled in size a pin’s head. By breaking the shell he dislodged nine worms, all of which were contained within one involucre.

† The system of the ovarists has been adopted by Harvey, Steno, Malpighi, Valignieri, Duhamel, Nuck, Littré, Swammerdam, Haller, Spallanzani, Bonnet, &c. It must not be forgotten that there are some animals that cannot be called *oviparous* in whatever signification the term may be received ; the *hydra*, for instance, multiplies its species by sending off shoots from its own body, and may, therefore, be said to be *gemmiparous*.

‡ Some fish are *viviparous*, e. g. *Muræna Anguilla*, or eel, *Blennius Viviparus*, &c. Among the amphibia we may notice the viper, which brings forth its young alive, and hence probably derives its name, *quod vivum pariat*. Spallanzani considers also the production of frogs as being rather of a *viviparous* than *oviparous* nature ; this judgment, however, of the future animal certainly partakes as much of the nature of an egg as of a fœtus, and may probably be considered as a connecting link between the two great classes. Insects likewise present us with exceptions, and several whimsical varie-

agate their species by the intervention of the egg, nor is such a mode of generation either accidental in its occurrence, or unimportant in its operation; had the winged inhabitants of the air been *viviparous*, the burden of gestation would have impeded the action of their wings, and have so far increased their gravity as to have rendered them incapable of the exertion of flight: the rigid and unpliant coverings of *crustaceous* animals would have opposed the expansion necessary for the developement of a *foetus*; and it is evident from the structure and habits of the tribe of serpents, that if they had been *viviparous*, their offspring must have suffered materially from the tortuous flexions, and friction necessarily attendant upon their progressive motions; and, lastly, the *multiparous** nature of insects and fishes at once convinces how improvident it would have been to engender them by any other mode than that which nature employs.

The eggs of the *ovipari* admit of an evident division into two classes, which I shall denominate, 1. *The Perfect*; and 2. *The Imperfect*. The former, which are deposited by the *aves*, and some genera of *amphibia*, are completely covered by a hard *shell*, or membrane, and receive no additions after their exclusion; while the latter are deposited by most *pisces*, and in general constitute a soft mass (*fluvago*), not being protected by any external involucre. The observations contained in this memoir relate more particularly to the egg of birds: their history, however, comprises whatever is interesting or important in the germs of inferior animals.

In order that I may be better able to form a systematic relation of those new facts and opinions which I wish to submit to

ties; the *aphides* lay eggs at the end of autumn, when the young produced from them in the spring are *viviparous* during the whole summer! The *cocci* hatch their eggs before their exclusion, and the young force their way through the abdomen of their mother. The *ovisci* carry their eggs in a particular receptacle, from which, in process of time, the young make their escape. The *hippobosca* brings forth neither eggs, nor larvæ, but those already gone into the *pupa* state! The *lacerta salamandra* brings forth young, complete in every part, but still enclosed in an egg.

* Reaumur ascertained that a single queen bee had laid in the months of March and April 12,000 eggs; and Lewenhoeck found that the *musca carnaria* deposited 144 eggs, from which, in one month, were produced as many flies; so that supposing one-half of these to be females, there would be in the third month 746,496 flies. The amazing fertility of fish may be illustrated by the *gadus morhua*, the cod fish, which will deposit among the rocks 9,000,000, or 10,000,000 of eggs; and again, the *perca fluviatilis* produces in April and May not less than 300,000 ova! Dr. Bastar says that he counted 12,144 eggs under the tail of a female lobster, besides those that remained in the *ovarium* unprotruded. I have frequently examined the ova of the lobster, and there is one circumstance that, perhaps, deserves notice; each ovum upon examination will be found to be hexagonal in its form. If we were inclined to be eager on the subject of final causes, we might at once conclude that this form was the one adopted, as affording facility of packing the greatest number in the least possible space, and we might adduce the structure of the honeycomb as an illustration: the fact, however, is simply this; that the ova, like all yielding bodies, assume the polyangular form, from the mutual pressure which each sustains from its neighbour. Every person must have observed the hexagonal bubbles upon the surface of porter and other liquors, from a similar cause.

your notice, I shall briefly relate the successive operations by which the egg is formed in the body of the animal ; the necessity of more minute detail is superseded by the valuable descriptions of Harvey and Malpighi.

The rudiments of the *ovum* are first visible in the *ovarium*, which, in fact, is nothing but a congeries of *vitelli*,* or yelks, attached to the spine by a proper membrane ; this repository is denominated by Fabricius, the *vitellarium*, or *vitellorum racemus*, and may be considered as analogous to the ovarium of the mammalia, or to the roe of fishes. These *vitelli* generally vary in progression from the size of a millet seed to that of an acorn ; each of which, according to its maturity, is successively detached from the rest, whence it descends a tube, called, from its resemblance to a funnel, *infundibulum*, and arrives at the *uterus*, the internal surface of which is extended by spiral convolutions ; here the albuminous fluids are secreted, and transmitted to the *vitellus* during its passage to the *fundus uteri*, or *cloaca*, where it receives its last addition, the external crust, or shell.

The egg thus formed and completed possesses every essential for its subsequent maturation, and requires only the emphatical energy of heat for the developement of its embryo ; this is conveyed through different media in the different classes of animals. In birds it is applied by incubation,† but in the *amphibia* and other animals, the heat of whose bodies is inconsiderable and inefficient, the eggs are deposited in mud or sand, or are exposed to the rays of the sun, by whose prolific influence myriads of beings are daily called into life and activity ; or they are placed in other favourable situations, all of which are too well known to the disciple of Linnæus to require any particular notice. It is, however, worthy of remark, that the medium through which heat is applied is suitably varied in the same species in different climates. In Senegal, for instance, the ostrich abandons her eggs to be hatched by the burning sands, while in the more temperate and congenial regions of the Cape of Good Hope, like other birds, she is inclined to incubation.

The different species of *astrus* will afford us an illustration of the variety of situations in which the insect tribe deposit their ova ; in which they are universally directed by an instinct to ensure a suitable temperature, and appropriate nutriment for the young brood ; thus the *astrus hamorrhoidalis* deposits them in the rectum of the horse, and the *Æ. ovis* in the frontal sinus of sheep, &c.

* *Vitellus*, derived à *vita*, because it contains the embryo.

† There are also other animals that accelerate the evolution of their ova by incubation. Thus bees in a hive generate a considerable quantity of heat without which their eggs would perish ; and the *testudo mydas*, or common turtle, deposits her eggs in the sand, and incubates during the night.

The parts of which the perfect egg consists are : 1. *Vitellus*, or yelk, with its *capsule* and *cicatricula* ; 2. *Albumina*, with their proper membranes ; 3. *Chalazæ* ; 4. *Folliculus aeris* ; 5. *Common membranes* ; 6. *Exterior involucrum*, or shell ; to each of which I shall successively direct my attention.

The *vitellus*, or yelk, is the part formed in *vitellario*, and is a yellow fluid contained in a membranous capsule, on which a greyish-white circular disk is discernible ;* this is named *cicatricula*, and is the speck of entity, the germ that is to be developed into the animal. “ *In hujus gratiam*,” says Malpighi, “ *reliqua comproducta videntur*.” We have here then arrived at the earliest stage in which we can detect the existence of the embryo. Our imperfect faculties will not enable us to ascend further, and yet, even now, the body is formed as the experiments and observations of Malpighi and Buffon most satisfactorily testify. The yelk is surrounded by a more tenacious fluid, of a light-straw colour, to which the name *albumen*, or more commonly *the white*, has been assigned ; this may easily be divided into two separate and distinct portions, each of which is contained in a concentric membrane. They differ from each other considerably in specific gravity, and seem to answer different purposes in the economy of the egg : the consistence of that which is exterior is far less than the one which immediately envelops the yelk, and is consumed in the earlier periods of incubation ;† while the internal and more viscid *albumen* seems reserved for the latter stages, when the chick must require a greater proportion of *generative matter* than at any other period of its evolution.

Many of the ancient philosophers imagined that the chick was formed out of the yelk, and that the white afforded nutriment. Such a theory, however, must be at once abandoned, when it is known that the *vitellus* suffers no other change by incubation than a degree of liquefaction, and that it is drawn up into the small intestines of the animal,‡ by means of an appropriate duct § (*ductus stemonis*) just before its exclusion. It is then evident that

* Fabricius supposed it to be a vestige of the ruptured *pedunculus*, or that portion of membrane by which each yelk is connected to the *vitellarium* ; and Æmiliius Parisanus contends that it is the semen of the male.

† There is a considerable difference discoverable in the milk of mammiferous animals at different periods subsequent to parturition. Fourcroy ascertained that it is most charged with calcareous phosphates immediately after parturition, and that the proportion of them gradually diminishes.

‡ The vitellus appears to be consumed in the first 10 days after the animal is hatched.—Monro.

§ Mr. Macartney observes, that the duct by which the yelk communicates with the intestine of the chick does not become entirely obliterated, but leaves a small sac, which remains during the life of the animal. In the snipe, this appendage is of considerable bulk, and on examining its internal structure, we shall find that it is lined with a kind of villous coat, and that it has numerous folds, or projections, which indicate that it possesses a glandular structure, exhibiting a curious example of the economy of nature in adapting an organ of foetal life to the exercise of a particular function in the full grown bird.

the albuminous portions * furnish materials for its evolution, while the *vitellus* is designed to administer support, until its digestive organs can gain sufficient powers to perform their functions, and the beak a degree of firmness adequate to withstand the hardness of its natural food. "*Ipsum animal*," says Pliny, "*ex albo liquore ovi corporatur, cibus ejus in luteo est*." The albumina, however, besides the office thus assigned to them, discharge another important duty, that of retaining by their non-conducting powers the vital temperature of the *cicatricula*; the *vitellus* also would seem to answer some other purpose, or why should it be necessary to those birds † whose parents so sedulously supply them with nourishment?

At each end of the egg, a white, shining, semipellucid body is inserted into the capsule of the yelk, which extends into the *albumen* in which it floats. These bodies, from their supposed resemblance to *hail*, have gained the name of *chalazæ*, or *grandines*, and, from having been formerly regarded as the sperm of the male bird, that of *treddles*. Bellini ‡ supposes that they are composed of numerous canals, which open into the *amnios*, or *cicatricula*, and send out their roots into the *white* for the purpose of forming a communication between them. Dr. Monro, § however, observes, that "if they be canals, they cannot have the least communication with the cavity in which the chick resides at any time, or in any state of the egg, otherwise than as they are both adhering to the membrane of the *vitellus*, upon which, or within which, no particular fibres, no canals, are stretched to the *cicatricula*." "The *chalazæ*," says Harvey, "appear to be the poles of the microcosm, and serve to connect the different parts of the egg, and to retain them in their due position. In addition to such an office, Derham ingeniously conjectures that, as they divide the yelk into two distinct and unequal hemispheres, they must preserve the *cicatricula* (let the position of the egg be what it may) in the same situation; for since the *chalazæ* are specifically lighter than the *white*, the yelk is kept buoyant, and the *cicatricula*, as it resides in the smaller hemisphere, will be always uppermost: this, in my opinion, is the true theory of the use of the *chalazæ*; for such a structure will not only preserve the *cicatricula* from the dangers of concussion, but by regulating its distance from the source of heat, it will ensure for it a more completely uniform temperature than could otherwise happen, and which is so essential to the evolution of

* The ingenious experiments of Mr. Hatchett seem to show that *albumen* is the parent fluid from which other animal principles may be derived: he accordingly found that it was convertible into *gelatine* and *fibrine*.

† Pigeons, for example, whose crops John Hunter ascertained to secrete a peculiar fluid during the breeding season for the sustenance of their young.

‡ Bellini de Motu Cordis, prop. ix.

§ Monro. See his works published by his son, Edin. 1781.

the animal, that the smallest irregularity overthrows the nice balance of the different actions that are to mature it, and produces fatal effects. So solicitous, therefore, was Nature to rescue the germ from the consequences of cold, that she has ordained other provisions, which seem as effective as the *chalaxa*, for the preservation of a due temperature. Thus is the *cicatricula* on all sides surrounded by fluids which are extremely feeble conductors of heat; these must necessarily retard the escape of caloric, and prevent the otherwise destructive chills which the occasional absence of the parent might induce. The eggs of other animals appear to be protected by an analogous apparatus. Thus the ova of frogs, and some other amphibia, are enveloped in spheres of mucilage, which the experiments of Spallanzani show to be essential, as he found that when this gluten was removed, the egg immediately perished.* It is certainly true that those fishes who retain their vitality long after their removal from the water, as eel and tench, have the power of secreting a slimy fluid, with which they envelope their bodies; while, on the contrary, those who, when drawn on shore, quickly die, as, for instance, mackerel, possess no such faculty, or, at least, only in a small degree. Is it not, therefore, extremely probable, that this albuminous matter, by repressing evaporation, and preventing, like the fluids of the egg, the escape of heat by its nonconducting nature, is the principal cause of this peculiar tenacity of life; perhaps a prodigious accumulation of fat may also, under certain circumstances, have a share in producing this effect; the *silurus glanis*, which is the fattest of all fresh water fishes, for it grows to the weight of 300 lb. lives very long after being taken out of the water.

Besides fishes, there are other animals who protect themselves from an excess of heat, or cold, by ejecting fluids from the surface of their bodies. The common snail is indebted to its profusion of slime for its power of resisting cold. The fable of the salamander being indestructible by fire, owes its origin to the faculty which this animal possesses of discharging from the numerous pores which are scattered over the surface of its body, a milky fluid, by which it defends itself for a short time against the fury of the flames. There is an account in the Phil. Trans. of a knight, at Rome, who cast a salamander in the fire, which it put out twice, and lived nine months afterwards!

The hen bird seems instinctively conscious of the mischief that would accrue from an irregular or diminished temperature. She is often seen to make use of her bill to push to the outer part of the nest those eggs that were nearest the middle, and to bring into the middle such as lay nearest the sides. The Egyp-

* This gluten is not of the same consistence in all the amphibia: it is, for instance, more abundant and viscid in frogs and toads than it is in lizards and newts.

tians, however, by a nice adjustment of their ovens, or *mamals** as they are called, succeed in hatching a great proportion of the eggs entrusted to their care by artificial heat. The celebrated Reaumur introduced the method into France; and Sir James Hall invented a regulating stove by which an equable temperature might be easily procured for the same purpose. During the period that I was at college, the late Sir Busick Harwood, the ingenious Professor of Anatomy in the University of Cambridge, frequently attempted to develope the egg by the heat of his hot-bed; but he only raised monsters, a result which he attributed to the unsteady application of heat.† It must, however, be observed, that deviations from the correct temperature are injurious and fatal only in proportion to the grade of vital energy which the ovular embryo possesses. Thus we learn, from the experiments of Spallanzani,‡ that the eggs of insects are better able to sustain the vicissitudes of temperature than those induced with more exalted vitality. Thus it is that the eggs of cold-blooded animals bear with impunity such an increase or decrease of temperature as is sufficient to destroy the animals themselves; for Spallanzani found *tadpoles* and frogs perished at 110°, but their eggs only at 133°.

If we pursue this inquiry, and quitting the animal kingdom descend into the scale of vegetable existence, where the energies of vitality are still more feeble and obscure, we shall find the same relative power of sustaining heat or cold between the plant and the seed, as I have stated to exist between the animal and its egg.

With respect to the relative destructive influence of vicissitude of temperature upon the egg of birds in different stages of developement, it would appear, from the interesting experiments of Reaumur, that it is more destructive in the earlier stages of incubation, especially diminution of temperature, but that increased heat is more injurious in the advanced states of developement.

After having related the agencies of heat and cold, I may mention that light has also been found by Michelotti, § of Turin,

* The inhabitants of the single village Berme, situated in the Delta, about 20 leagues from Cairo, among whom this art is alone practised, give life by means of their *namals* to two-thirds of the eggs entrusted to their care, amounting in one season, which continues but for six months, to the astonishing sum total of 92,600,000. Corneille le Bruyn, tom. ii. has collected the observations of many travellers on this subject. Father Sichard also gives us an interesting account of the same art; and Reaumur has published a very complete work, illustrated with numerous engravings.

† The ancients were acquainted with the possibility of hatching eggs artificially. Pliny (lib. x. cap. 55) says, that eggs laid upon beds of straw in a warm place, and after being regularly turned from time to time, would, at the proper period, disclose the included animal. Pliny moreover states, that Livia hatched a chicken by the warmth of her bosom. Gesner and Aldrovandus have collected the passages of the ancients, and those of the authors of their own time, that mention the method of hatching eggs by dung.

‡ Spallanzani. Tracts on the Nature of Animals and Vegetables.

§ Journal de Physique, Ventose, An. ix.

to exert a decided influence on the ova of animals; he placed the eggs of different species of *phalanæ* in transparent and opaque jars, when he uniformly found that those in the black jars were first hatched; he, therefore, concludes that light is prejudicial to the developement of the egg: thus we find, says he, that the eggs of many birds are furnished with an opaque shell, as those of birds, and that if it be delicate, the parent deposits them in dark and concealed places." Before any conclusion can be legitimately deduced from the experiments of Michelotti, it ought to be shown that the temperature in both jars was the same; for unless this were established by actual experiment, we might be inclined to draw an opposite inference from the different radiating powers of black and white surfaces.

(To be continued.)

ARTICLE V.

On the Expansion of the Functions $f(x)$, $f(x, y)$, $f(x, y, z)$, &c.
By Mr. James Adams.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Stonehouse, near Plymouth, March 5, 1821.

THE usual method of expanding the functions $f(x)$, $f(x, y)$, $f(x, y, z)$, $f(x, y, z, v)$, &c. being from their nature very troublesome, I beg to recommend the following, which, I trust, will be found more convenient and expeditious; the insertion of which in the *Annals of Philosophy* will oblige, Sir,

Your humble servant,

JAMES ADAMS.

Problem 1.—It is required to develope $f(x)$, or any function of x .

Let $f(x) = u$; then place the symbols d , d^2 , d^3 , &c. successively before the character u ; likewise the symbols d and Δ successively before the assumed series $x + x^2 + x^3 + x^4 + \&c.$ together with the corresponding numerical coefficients in the following manner:

$$\frac{d u}{1 \cdot d x} \Delta x + \frac{d^2 u}{1 \cdot 2 d x^2} \Delta x^2 + \frac{d^3 u}{1 \cdot 2 \cdot 3 d x^3} \Delta x^3 + \&c. \text{ we shall then}$$

$$\text{have } f(x + \Delta x) = u + \frac{d u}{1 \cdot d x} \Delta x + \frac{d^2 u}{1 \cdot 2 d x^2} \Delta x^2 + \frac{d^3 u}{1 \cdot 2 \cdot 3 d x^3} \Delta x^3 + \&c.$$

Problem 2.—It is required to develop $f(x, y)$, or any function of x and y .

Let $f(x, y) = u$; then place the symbols d, d^2, d^3 , &c. successively before the character u ; likewise the symbols Δ and Δ before the following combinations of x and y ; together with the corresponding numeral coefficients in the following manner, omitting the coefficients and signs in the expansions of $(x + y)$.

$(x + y)$	Terms corresponding to $d^0 u$. (A.)	Terms corresponding to $(x + y)^2$	Terms corresponding to $d^2 u$. (B.)	$(x + y)^3$	Terms corresponding to $d^4 u$. (C.)	$(x + y)^4$	Terms corresponding to $d^4 u$. (D.) &c.
x	$\frac{d^0 u}{d^0 x} \Delta x$	x^2	$\frac{d^2 u}{1 \cdot 2 d^2 x^2} \Delta x^2$	x^3	$\frac{d^2 u}{1 \cdot 2 \cdot 3 d^2 x^3} \Delta x^3$	x^4	$\frac{d^4 u}{1 \cdot 2 \cdot 3 \cdot 4 d^4 x^4} \Delta x^4$
y	$\frac{d^0 u}{d^0 y} \Delta y$	xy	$\frac{d^2 u}{d^2 x \cdot d^2 y} \Delta x \cdot \Delta y$	$y x^2$	$\frac{d^2 u}{1 \cdot 2 \cdot d^2 y \cdot d^2 x^2} \Delta y \cdot \Delta x^2$	$y x^3$	$\frac{d^4 u}{1 \cdot 2 \cdot 3 d^2 y \cdot d^2 x^2} \Delta y \cdot \Delta x^3$
		y^2	$\frac{d^2 u}{1 \cdot 2 d^2 y^2} \Delta y^2$	$x y^2$	$\frac{d^2 u}{1 \cdot 2 d^2 x \cdot d^2 y^2} \Delta x \cdot \Delta y^2$	$x^2 y^2$	$\frac{d^4 u}{(1 \cdot 2)^2 \cdot d^2 x^2 \cdot d^2 y^2} \Delta x^2 \cdot \Delta y^2$
				y^3	$\frac{d^2 u}{1 \cdot 2 \cdot 3 d^2 y^3} \Delta y^3$	$x y^3$	$\frac{d^4 u}{1 \cdot 2 \cdot 3 d^2 x \cdot d^2 y^3} \Delta x \cdot \Delta y^3$
						y^4	$\frac{d^4 u}{1 \cdot 2 \cdot 3 \cdot 4 d^4 y^4} \Delta y^4$

From whence it appears that $f(x + \Delta x, y + \Delta y) = u + A + B + C + D + \&c.$

Problem 3.—It is required to develop $f(x, y, z)$ or any function of x, y , and z .

Let $f(x, y, z) = u$; then place the symbol d, d^2, d^3 , &c. successively before the character u ; likewise the symbols Δ and Δ before the following combinations of x, y , and z ; together with the corresponding numeral coefficients in the following manner, omitting the coefficients and signs in the expansions of $(x + y + z)$.

$(x+y+z)$	Terms corresponding to $\frac{d^3 u}{d^3 x}$ (A.)	Terms corresponding to $\frac{d^3 u}{d^3 x}$ (B.)	$(x+y+z)^3$	Terms corresponding to $\frac{d^3 u}{d^3 x}$ (C.)	$(x+y+z)^4$	Terms corresponding to $\frac{d^4 u}{d^4 x}$ (D.)
x	$\frac{d^3 u}{d^3 x} \Delta x$	$\frac{d^3 u}{d^3 x} \Delta x^2$	x^3	$\frac{d^3 u}{d^3 x} \Delta x^3$	x^4	$\frac{d^4 u}{d^4 x} \Delta x^4$
y	$\frac{d^3 u}{d^3 y} \Delta y$	$\frac{d^3 u}{d^3 y} \Delta x \cdot \Delta y$	yx^3	$\frac{d^3 u}{d^3 y} \Delta y \cdot \Delta x^2$	$y x^3$	$\frac{d^4 u}{d^4 y} \Delta y \cdot \Delta x^3$
z	$\frac{d^3 u}{d^3 z} \Delta z$	$\frac{d^3 u}{d^3 z} \Delta x \cdot \Delta z$	xz^3	$\frac{d^3 u}{d^3 z} \Delta x \cdot \Delta y^2$	$x^2 y^2$	$\frac{d^4 u}{d^4 z} \Delta x^2 \cdot \Delta y^2$
		$\frac{d^3 u}{d^3 z} \Delta y^2$	y^3	$\frac{d^3 u}{d^3 z} \Delta y^3$	$x y^3$	$\frac{d^4 u}{d^4 z} \Delta x \cdot \Delta y^3$
		$\frac{d^3 u}{d^3 z} \Delta y \cdot \Delta z$	zx^2	$\frac{d^3 u}{d^3 z} \Delta z \cdot \Delta x^2$	y^4	$\frac{d^4 u}{d^4 z} \Delta y^4$
		$\frac{d^3 u}{d^3 z} \Delta z^2$	xyz	$\frac{d^3 u}{d^3 z} \Delta x \cdot \Delta y \cdot \Delta z$	zx^3	$\frac{d^4 u}{d^4 z} \Delta z \cdot \Delta x^3$
			xy^2	$\frac{d^3 u}{d^3 x} \Delta x \cdot \Delta y^2$	$yx x^2$	$\frac{d^4 u}{d^4 x} \Delta y \cdot \Delta x \cdot \Delta x^2$
			xz^2	$\frac{d^3 u}{d^3 x} \Delta x \cdot \Delta z^2$	$x x y^2$	$\frac{d^4 u}{d^4 x} \Delta x \cdot \Delta z \cdot \Delta y^2$
			yz^2	$\frac{d^3 u}{d^3 y} \Delta y \cdot \Delta z^2$	$z y^3$	$\frac{d^4 u}{d^4 y} \Delta z \cdot \Delta y^3$
			z^3	$\frac{d^3 u}{d^3 z} \Delta z^3$	$x^2 x^2$	$\frac{d^4 u}{d^4 z} \Delta x^2 \cdot \Delta z^2$
					$xy x^2$	$\frac{d^4 u}{d^4 x} \Delta x \cdot \Delta y \cdot \Delta x^2$
					$y^2 x^2$	$\frac{d^4 u}{d^4 y} \Delta y^2 \cdot \Delta x^2$
					$x x^2$	$\frac{d^4 u}{d^4 x} \Delta x \cdot \Delta x^3$
					$y y^2$	$\frac{d^4 u}{d^4 y} \Delta y \cdot \Delta y^3$
					z^4	$\frac{d^4 u}{d^4 z} \Delta z^4$

Hence we obtain $f(x + \Delta x, y + \Delta y, z + \Delta z) = u + A + B + C + D + \&c.$

In like manner, we may expand the function $f(x, y, z, v)$, for the several combinations of the variables abstracting the numeral coefficients are as follow:

$$(x + y + z + v) = x + y + z + v$$

$$(x + y + z + v)^2 = x^2 + xy + xz + y^2 + xv + yz + z^2 + yv + zv + v^2$$

$$(x + y + z + v)^3 = x^3 + xy^2 + xz^2 + xv^2 + y^3 + yx^2 + yz^2 + yv^2 + z^3 + \&c. \text{ to 20 terms.}$$

$$(x + y + z + v)^4 = x^4 + xy^3 + xz^3 + xv^3 + y^4 + yx^3 + yz^3 + yv^3 + z^4 + \&c. \text{ to 35 terms.}$$

$\&c.$

By prefixing the symbols $\Delta, d, d^2, d^3, \&c.$ before the variables, x, y, z, v , and u , as in the preceding problems, we readily obtain the expansion of $f(x, y, z, v)$, and so on for a greater number of variables.

In the foregoing expansions, it will be observed that the index of the symbol d placed before u is equal to the sum of the indices in the several combinations of the variables, the symbols d and Δ are simply placed before the variables and their powers, and the numeral coefficients in the denominators are also governed by the indices of the variables. Thus the differential coefficient

of $x^2 y^3 z^4$ will be found to be $\frac{d^{2+3+4} u = d^9 u}{(1 \cdot 2)(1 \cdot 2 \cdot 3)(1 \cdot 2 \cdot 3 \cdot 4) d x^2 \cdot d y^3 \cdot d z^4}$ and the differential coefficient of $x^m y^n z^r$, or of u , will be

$\frac{d^{m+n+r} u}{(1 \cdot 2 \dots m)(1 \cdot 2 \dots n)(1 \cdot 2 \dots r) d x^m \cdot d y^n \cdot d z^r}$, a general form for three variables, which may be extended at pleasure.—(See Woodhouse's "Analytical Calculations," p. 86.)

The *differential* of a function being the second term of its developement, or that part of the expansion of a function which contains the first powers only of the increments $dx, dy, dz, \&c.$ It will be perceived that the differential of $f(x)$ consists of one term of $f(x, y)$ of two terms, of $f(x, y, z)$ of three terms, $\&c.$ Therefore by changing Δx into $dx, \Delta y$ into $dy, \Delta z$ into $dz, \&c.$ we have

$$df(x) = du = \frac{du}{dx} dx, df(x, y) = du = \frac{du}{dx} dx + \frac{du}{dy} dy,$$

$$df(x, y, z) = du = \frac{du}{dx} dx + \frac{du}{dy} dy + \frac{du}{dz} dz, \&c.$$

$\frac{du}{dx} dx, \frac{du}{dy} dy, \&c.$ are called partial differentials, and

$\frac{du}{dx}, \frac{du}{dy}, \frac{du}{dz}, \&c.$ are called differential coefficients.

ARTICLE VI.

Observations on Dr. Prout's Estimate of Mortality from the Operation of Lithotomy. By John Yellowly, MD. FRS. &c.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Carrow Abbey, Norwich, March 30, 1821.

I BEG to notice, through the *Annals of Philosophy*, an inaccuracy into which Dr. Prout has inadvertently fallen, in the statement which he has given, in his valuable work on the Diseases of the Urine,* of the mortality occurring in the operation of lithotomy, in the Norfolk and Norwich Hospital; and in the deduction which he has made, as to the mean ratio of mortality from that operation, over the whole kingdom.

Dr. Prout quotes Dr. Marcet's published account of the cases which occurred in the Norfolk and Norwich Hospital up to 1816,† and infers, that as the mortality which took place in individuals below puberty, was 1 in 18, while that in adults, was 1 in $4\frac{3}{4}$, the mortality of the whole must be 1 in $11\frac{3}{8}$, or the mean between the two proportions.

It is obvious, however, that this calculation could only be correct, if the number of deaths at those two respective periods were equal; but as this is not the case, the mean ratio can only be obtained, by dividing the whole number of cases, by the whole number of deaths, which Dr. Marcet does, and thus gives the mortality as 1 in $7\frac{1}{4}$, the deaths being 70 in 506 cases.

Taking this as the accurate proportion of deaths in the Norfolk and Norwich Hospital, up to the year 1816, (which, from my own inspection I can state to be the case,) the proportional mortality, from the operation of lithotomy in the whole kingdom, as inferred by Dr. Prout, from the mean of that in the Bristol, Leeds, and Norwich Hospitals, will be about 1 in $6\frac{1}{4}$, instead of 1 in $7\frac{3}{4}$, which he states it to be.

I remain, dear Sir, yours sincerely,

J. YELLOWLY.

* Page 218.

† Marcet on Calculous Disorders, p. 26.

ARTICLE VII.

Results of a Meteorological Journal kept at the Observatory of the Academy, Gosport. By William Burney, LL.D.

Lat. 50° 47' 58" North. Long. 1° 6' 4" West of Greenwich. In Time, 4' 24.3"

1820.	BAROMETER.							DAY AND NIGHT THERMOMETER.							DE LUC'S HYGROMETER.										
	Months.	Max.	Min.	Med.	Range.	Number of changes.	Spaces described.	Greatest var. in 24 hours.	Med. at 8, a. m.	Med. at 2, p. m.	Med. at 8, p. m.	Max.	Min.	Mean range of the index.	Med. at 2, p. m.	Med. at 8, a. m.	Med. at 8, p. m.	Med at 2, p. m.	8, a. m. & 8, p. m.						
Jan.	30.72	28.57	29.858	2.15	24	9.68	0.90	39.85	29.87	29.86	29.86	53	14	34.42	38	19	37.52	32.74	33.90	100.56	44	75.5	84.0	83.8	81.1
Feb.	30.30	29.39	29.945	0.91	22	4.52	0.45	29.94	29.94	29.95	29.95	52	26	39.27	26	18	43.21	36.31	38.48	99.50	49	72.9	83.2	81.7	79.2
March. ...	30.36	28.78	29.818	1.58	29	8.03	0.86	29.91	29.91	29.92	29.92	64	25	42.74	39	23	48.55	39.81	41.13	100.39	61	58.3	70.3	72.3	66.9
April.	30.52	29.17	29.956	1.35	26	5.42	0.54	29.96	29.95	29.96	29.96	69	85	50.70	34	29	58.17	49.80	48.46	94.38	56	53.1	63.8	67.3	61.4
May.	30.34	29.02	29.786	1.32	18	6.14	0.80	29.80	29.79	29.78	29.78	72	37	55.95	35	24	61.84	56.07	52.22	100.40	60	50.9	56.8	67.2	58.3
June	30.40	29.55	29.956	0.85	22	4.44	0.36	29.95	29.96	29.96	29.96	87	40	60.45	47	30	68.16	60.97	58.60	95.34	61	44.8	50.2	58.9	51.3
July	30.23	29.34	29.949	0.89	21	3.54	0.42	29.95	29.95	29.95	29.95	78	47	63.66	31	27	71.03	63.12	64.84	77.33	44	44.1	51.6	59.8	51.8
August ...	30.32	29.54	29.920	0.78	26	5.21	0.37	29.91	29.92	29.92	29.92	77	41	61.00	36	26	69.16	61.13	59.97	100.37	63	49.1	50.7	67.0	58.6
Sept.	30.40	29.46	30.025	0.94	21	5.35	0.52	30.02	30.03	30.02	30.02	71	34	56.73	37	26	63.57	55.87	54.76	100.34	68	52.0	66.8	72.2	63.6
October..	30.59	28.40	29.527	2.19	22	9.08	0.98	29.52	29.52	29.52	29.52	63	38	49.79	25	23	54.71	48.23	47.74	92.84	58	54.9	67.5	68.7	63.6
Nov.	30.24	29.40	29.818	0.84	20	5.45	0.56	29.82	29.81	29.83	29.83	59	31	44.50	28	22	48.00	43.03	43.80	100.55	45	69.5	80.8	81.5	77.3
Dec.	30.34	29.37	29.918	0.97	20	4.79	0.44	29.92	29.94	29.92	29.92	55	23	41.77	32	14	43.42	40.90	41.39	100.56	44	75.6	80.1	79.8	78.5
For 1820	30.72	28.40	29.873	14.77	271	71.65	0.98	29.88	29.88	29.88	29.88	87	14	50.13	34.0	30	55.61	49.00	48.77	100.33	54.2	58.4	67.9	71.7	65.9
For 1819	30.50	29.09	29.881	11.03	280	65.19	0.83	29.87	29.87	29.87	29.87	85	15	52.10	32.3	30	57.62	50.53	50.84	100.39	47.4	67.8	76.3	77.1	73.7
For 1818	30.60	28.50	29.881	12.59	271	73.60	1.00	29.87	29.87	29.87	29.88	91	23	52.79	32.8	32	59.30	51.80	51.42	100.29	48.0	61.6	69.3	69.7	66.8

1820.	A SCALE OF THE WINDS.								CLOUDS.							WEATHER.				ATMOS. PHENOMENA.								Rain, hail, snow, and sleet, in inches, &c.					
	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Total number of days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	A clear sky.	Fair with clouds.	An overcast sky.	Fog.	Rain.	Anthelion.	Parhelia.	Paraselenae.	Solar halos.	Rainbows.	Small meteors.		Lightning.	Thunder.	Evaporation in inches, &c.		
January.	5	2	7	1	3	4 $\frac{1}{2}$	5	3 $\frac{1}{2}$	31	12	12	22	3	3	17	17	2	15 $\frac{1}{2}$	4	4	9	0	0	0	0	2	4	0	7	0	0	0.35	
February.	4	3 $\frac{1}{2}$	7 $\frac{1}{2}$	4	3	2 $\frac{1}{2}$	3	11 $\frac{1}{2}$	29	7	10	16	3	3	6	18	12	11	101	4	5	0	0	0	0	2	0	0	2	0	0	0.75	
March...	6 $\frac{1}{2}$	4	3 $\frac{1}{2}$	8	1 $\frac{1}{2}$	3 $\frac{1}{2}$	6 $\frac{1}{2}$	3	31	14	11	23	2	13	15	15	4	15	70	5	5	0	4	0	0	2	0	0	1	0	0	1.75	
April....	4 $\frac{1}{2}$	2 $\frac{1}{2}$	6	1	3	3	7	3	30	20	18	18	1	18	12	10	6	16	20	20	6	0	4	0	0	1	1	0	1	0	0	3.40	
May	4	0	2 $\frac{1}{2}$	2 $\frac{1}{2}$	7 $\frac{1}{2}$	12	4	2	31	28	22	29	0	27	13	16	1	16	50	9	0	4	1	0	1	1	1	2	1	1	1	3.60	
June	1	2	2	1 $\frac{1}{2}$	3	1	9	9 $\frac{1}{2}$	30	24	21	25	5	21	17	14	5	13	60	6	0	4	0	0	5	1	0	1	3	2	1	4.30	
July	3 $\frac{1}{2}$	1 $\frac{1}{2}$	4	3 $\frac{1}{2}$	3	1	8	6 $\frac{1}{2}$	31	23	24	28	5	20	21	10	3	19	50	4	0	4	0	0	3	1	0	15	4	1	0	3.90	
August...	2	2	2	1	2 $\frac{1}{2}$	7 $\frac{1}{2}$	9	5	31	23	22	29	5	19	14	16	2	15	60	5	0	5	0	0	3	1	0	80	4	1	4	4.25	
September.	3	2	3 $\frac{1}{2}$	3	2	4	6	7	30	21	19	24	10	22	15	13	2	18	21	7	0	4	0	0	3	2	1	10	1	0	0	3.35	
October..	1	7	2	2	2	3	6	8	31	12	13	25	6	16	18	16	3	13	50	5	10	0	4	0	2	3	0	4	1	1	2	4.10	
November	4	5	7	2	3 $\frac{1}{2}$	4	2 $\frac{1}{2}$	2	30	13	17	27	8	12	11	14	1	11	101	7	0	0	5	2	2	0	0	2	0	0	0	1.00	
December	1	6	7	1 $\frac{1}{2}$	2	5	6	2	31	11	10	17	2	9	22	15	1	7	142	7	1	1	1	5	1	1	0	5	0	0	0	0.60	
For 1820	35 $\frac{1}{2}$	38 $\frac{1}{2}$	54	26	36	51	71 $\frac{1}{2}$	53	366	208	199	283	50	186	188	170	32	169 $\frac{1}{2}$	76 $\frac{1}{2}$	83	1	34	10	39	24	14	13	14	6	29	35	25.75	
For 1819	45 $\frac{1}{2}$	44	49 $\frac{1}{2}$	29	35	34 $\frac{1}{2}$	87 $\frac{1}{2}$	60	363	209	199	294	39	215	188	189	33 $\frac{1}{2}$	164	66 $\frac{1}{2}$	99	2	38	7	32	25	18	12	16	12	31	26	33.33	
For 1818	25 $\frac{1}{2}$	38	47 $\frac{1}{2}$	26 $\frac{1}{2}$	54 $\frac{1}{2}$	82	68	43	365	134	100	200	22	133	100	160	41 $\frac{1}{2}$	178	38	61	101	3	36	2	42	23	10	31	15	12	49	80	27.94

ANNUAL RESULTS.

Barometer.

	Inches.
Greatest pressure of the atmosphere, Jan. 9. Wind E..	30.72
Least pressure of the atmosphere, Oct. 22. Wind W...	28.40
Mean range of the mercury.	2.32
Annual mean pressure of the atmosphere	29.873
Mean pressure for 180 days, with the moon in S. declination	29.897
Mean pressure for 186 days, with the moon in N. declination.	29.849
Annual mean pressure at eight o'clock, a.m.	29.887
_____ at two o'clock, p.m.	29.882
_____ at eight o'clock, p.m.	29.882
Greatest range of the mercury in October	2.190
Least ditto, ditto, in August	0.780
Greatest annual variation in 24 hours in October.	0.980
Least of the greatest variations in 24 hours in June	0.360
Spaces described by the oscillations of the mercury	71.650
Number of changes, caused by the variations in the weight of the atmospheric column	271

Day and Night Thermometer.

	Degrees.
Greatest thermometrical heat, June 26. Wind W.	87.00
_____ cold, Jan. 15. Wind N.E. . . .	14.00
Annual range of the thermometer between the extremes.	73.00
Annual mean temperature of the atmosphere	50.13
_____ at 8 a.m.	49.00
_____ at 8 p.m.	48.77
_____ at 2 p.m.	55.61
Greatest range in June.	47.00
Least of the greatest ranges in October.	25.00
Annual mean range	34.00
Greatest annual variation in 24 hours in June.	30.00
Least of the greatest variations in 24 hours in December	14.00

De Luc's Whalebone Hygrometer.

	Degrees.
Greatest humidity of the atmosphere, 15 times.	100.0
Greatest dryness of ditto on July 1 and 25.	33.0
Annual range of the index between the extremes.	67.0
Annual mean of the hygrometer at 8 a.m.	67.9
_____ at 8 p.m.	71.7

	Degrees.
Annual mean of the hygrometer at 2 p.m.	58.4
at 8, 2, and 8 o'clock. . .	65.9
Greatest mean humidity of the atmosphere in January ..	81.1
Greatest mean dryness of the atmosphere in June.	51.3

Position of the Winds.

	Days.
From North to North-east	35 $\frac{3}{4}$
North-east to East	38 $\frac{1}{2}$
East to South-east	54
South-east to South.	26
South to South-west	36
South-west to West	51
West to North-west.	71 $\frac{3}{4}$
North-west to North	53
	<hr/> 366

Clouds agreeably to the Nomenclature; or the Number of Days on which each Modification has appeared.

	Days.
Cirrus	208
Cirrocumulus.	199
Cirrostratus	283
Stratus.	50
Cumulus.	186
Cumulostratus	188
Nimbus	170

General State of the Weather.

	Days.
A transparent atmosphere, without clouds.	32
Fair, with various modifications of cloud	169 $\frac{1}{2}$
An overcast sky, without rain	76
Fog.	5 $\frac{1}{4}$
Rain, hail, sleet, and snow	83
	<hr/> 366

Atmospheric Phenomena.

	Number.
Anthelion, or mock-sun, diametrically opposite to the real Sun.	1
Parhelia, or mock-suns.	34
Paraselenæ, or mock-moons.	10
Solar halos.	39
Lunar halos.	24
Rainbows, perfect.	14

	Number.
Small meteors, or shooting stars	131
Lightning, days on which it occurred	14
Thunder, days on which it occurred	6

Evaporation.

	Inches.
Greatest quantity in June	4.30
Least quantity in January	0.35
Total amount for the year	29.35

Rain, &c.

	Inches.
Greatest quantity in October	4.10
Least quantity in March	0.46
Total amount for the year	25.75

N.B. The barometer is hung up in the Observatory, about 30 feet above high-water mark ; and the Self-registering Horizontal Day and Night Thermometer, and De Luc's Whalebone Hygrometer, are placed in open-worked cases, in a northern aspect, out of the Sun's rays, and 10 feet above the garden ground. The pluviometer and evaporator have respectively the same square area : the former is emptied every morning at 8, a.m. after a precipitation of rain, into a cylindrical glass gauge accurately graduated to 1-100th of an inch ; and the quantity lost by evaporation from the latter, is ascertained at least every third day, and sometimes oftener, when strong evaporations happen by means of a high temperature and very dry winds.

Of the Pressure.—The barometrical or mercurial column in the course of the year has shown a great elevation, and an unusual depression ; the former occurred in January, the latter in October, making its range about $2\frac{1}{2}$ inches. The mean pressure is 1-125th of an inch less than it was in either of the two preceding years ; but the average of all the monthly ranges $2\frac{1}{2}$ inches greater. The number of changes is nine less than in 1819, but it exactly agrees with that of 1818. For 180 days, while the moon was in south declination, the barometrical mean is about 1-20th of an inch higher than in the 186 days of her north declination. Last year the barometrical mean, or mean pressure, was greater while she was in north declination, than while she ranged in south declination, by 1-17th of an inch. This shows nearly an equal pressure of the incumbent atmosphere for her north and south declination during the last 26 lunations, and has but little tendency to establish "the periodicity of atmospheric changes," said to be influenced by the moon in the different parts of her complex orbit, unless the mean results should happen to come out more favourably to the hypothesis,

by confining the mean barometrical pressure to particular phases of the moon, which we have not now time to investigate. The aggregate of the spaces described by the curve was greatest in January, and least in July. The averages of all the observations at eight, two, and eight o'clock every day, precisely agree with each other, and are 1-100th of an inch less than those of the preceding year.

Temperature.—The mean temperature of this year is nearly 2° less than in 1819, $2\frac{1}{4}^{\circ}$ less than in 1818, and 0.41 of a degree less than in 1817; but about $1\frac{1}{4}^{\circ}$ higher than it was in the ungenial year 1816. The *maximum* of the two preceding years has fallen off, and the *minimum* this year is considerably lower than at any time since February, 1816. The mean heights of the thermometer at eight, a. m. and eight, p. m. correspond within one-fourth of a degree, and also with the annual mean temperature within $1\frac{1}{4}^{\circ}$ as usual; but the mean at two, p. m. is nearly 4° less than that at the same hour in the warm year 1818. Although the sun arrives at his greatest north declination at the summer solstice, when his meridional rays in this latitude are inclined to the earth at an angle of about $62^{\circ} 40'$, and have a powerful effect upon a thermometer exposed to them under a clear sky; yet a month is generally taken up in the exhalation of the earth's moisture to a certain depth, before we experience experimentally the greatest monthly mean temperature; because the earth and air do not receive their *maximum* heat till the latter part of July. At the winter solstice, the sun has his greatest south declination, when his rays are inclined to us at an angle of about $15^{\circ} 44'$, and have then the least power upon the thermometer; yet we do not experience the lowest monthly mean temperature till January; because the earth has not wholly parted with the heat it obtained in the summer till the middle of this month. An exception to these general rules, however, does sometimes occur at the retrogression of the seasons, arising from untimely weather.

De Luc's Whalebone Hygrometer.—The mean humidity of the air near the ground, as ascertained by three observations each day at eight, two, and eight o'clock, appears to be 1° (within 1-100) drier than the warm year 1818! This appears incongruous at first sight; but as the amount of rain and of evaporation is much less this year, the ascent of vapours must have been slower and in less quantities, and consequently the lower *stratum* of air drier upon the hygrometric substance of the instrument. The mean at eight, a. m. for the last three years, is 71.16° ; and the mean at eight, p. m. 72.83° .

Prevailing Winds.—In the table containing a scale of the winds, or the number of days on which they have prevailed, we see that they have blown more uniformly round the points of the compass this year than in the years 1818 and 1819. The winds to the west of the meridian have, however, prevailed longer than those on the eastern side, by about one-sixth of the whole year;

and those to the W and SW more than the one-third of the period, which we believe to be generally the case in the southern part of England. The winds from E and NW are nearly equal in their duration; and those from SE prevail a less time than any other. The NE and E winds prevail mostly in the winter months, and the westerly winds in the summer. The uncommon vicissitudes of weather, and anomalies of the seasons, chiefly depend upon the prevalence of particular winds, which long experience has taught us. That there is some uniformity in the duration and distribution of the winds round the compass in a series of years is demonstrated by the foregoing proximate scale. By a strict attention to the motions of the modifications of cloud, we have been recently enabled to detect more frequently the simultaneous existence of several currents of wind, sometimes *three*, and often *two*, crossing each other at right angles, at other times nearly in opposite directions. We have observed that the upper currents generally prevail over, and descend into the region of the lower ones after they have subsided. The number of strong gales of wind, or the days they have respectively prevailed this year, is as follows: N, 3; NE, 7; E, 8; SE, 2; S, 2; SW, 19; W, 8; NW, 3; Days, 52.

Clouds.—Some striking coincidences may also be seen in the table in the number of days on which each modification has appeared during the last two years, the *stratus*, *cumulus*, and *nimbus*, differing most in the days of their appearance; and the last two are less in number than in 1819, on account of the rains being less frequent. We have nothing particular to offer respecting the modifications of clouds, only that their assigned positions have been sometimes reversed in changeable weather. Some remarkable coincidences also appear in that part of the table under Weather, as in the number of clear, fair, and rainy days. It is necessary to observe here, that although we have allowed only 83 days for rain, yet a precipitation of rain, hail, or snow, has actually occurred 150 days out of the 366; but 83 whole days and nights is nearly the time it has rained throughout the year. A separation of the time has also been made for the appearances of other peculiarities of weather. This method, however, is attended with trouble; still we shall pursue it, as it is probable that some definite results may be obtained from it at a future period.

Atmospheric Phenomena.—Thunder and lightning have not occurred so often this year as the two preceding ones; but the number of small meteors, or shooting stars as they are commonly called, is greater—of 131, no less than 80 appeared in August. On the 9th of this month, between 11 and 12, p. m. 35 shot in different directions, three of them were caudated while passing between the constellations *Lyra* and *Ursa Major*, their sparkling trains having been left brilliantly illuminated for several seconds of time subsequent to the disappearance of the ignited bodies: this

indeed was the grandest display of meteors we ever remember to have seen in so short a period, arising from the very gaseous or inflammable state of the air. The solar and lunar halos, *parase-lenæ*, and *parhelîa*, are peculiar prognostics which seldom fail to indicate approaching wet weather. The time and appearance of these, and other atmospheric *phenomena*, may be seen particularly described in our daily remarks on the weather, already published. The amount of rain and of evaporation is less this year than it has been for some years past. March was the driest month, and August very wet and windy.

Variation of the Magnetic Needle.—Further observations on the diurnal motion of the magnetic needle have enabled us to determine its recession from its greatest western declination. In this latitude it first began to recede, but in a very slow manner, in the middle of the year 1819. The mean westerly variation here for December, 1820, was $24^{\circ} 31' 15''$.

The Planet Venus.—This brilliant planet excited much curiosity among the inhabitants of this town and neighbourhood on Sept. 19, 1820, when she was distinctly seen with the naked eye by people of all ages assembled in the streets, &c. from eight, a. m. till she was near the western horizon in the afternoon. She was seen by us, without the help of a telescope, on the following days this year:—Sept. 11, 12, 13, 14, 19, 26, 28, and 30; Oct. 2, 3, 5, 6, 7, 8, 15, 17, 19, 20, 24, and 30; Nov. 2, 7, 15, and 28. On the latter day she was $40^{\circ} 58' 45.7''$ to the west of the Sun's centre. In the morning of October 6, Venus was $46^{\circ} 15' 15''$ to the west of the Sun's centre, and on the 8th following, $46^{\circ} 25'$; the latter was the greatest distance that could be obtained from a number of sights taken with an excellent sextant this and the preceding day; consequently she was then at her greatest western elongation. From these and previous observations, it is inferred, that this planet may be seen with the naked eye in the open day, particularly when on the meridian, in all that part of her orbit from 42° on either side of the sun in clear weather. Should the atmosphere be clear at the close of next October, or the beginning of November, when she will be about 42° to the east of the sun, no doubt she may be then seen with the naked eye in the open day, and for some weeks afterwards.

ARTICLE VIII.

Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23' 30". Above the Level of the Sea 129 feet.

1820.	Morning, 8 o'clock.		Even., 10 o'clock.		Mean temp. by Six's Ther.	Depth of Rain. In. 100	No. of days.	
	Mean height of		Mean height of				Rain or Snow.	Fair.
	Barom.	Ther.	Barom.	Ther.				
Jan.	29.763	29.355	29.776	30.097	30.532	2.30	14	17
Feb.	29.883	38.655	29.875	39.069	40.310	1.40	12	17
March	29.761	32.355	29.762	39.645	41.451	0.50	10	21
April.	29.794	45.500	29.795	44.533	47.333	0.90	6	24
May.	29.632	49.516	29.602	47.870	50.741	5.20	19	12
June.	29.838	55.066	29.837	52.966	55.533	1.60	13	17
July.	29.844	57.549	29.788	55.560	59.322	1.80	9	22
Aug.	29.621	55.645	29.627	53.590	56.806	2.20	13	18
Sept.	29.792	51.100	29.777	50.060	52.633	1.20	12	18
Oct.	29.499	43.193	29.480	42.742	44.419	2.50	12	19
Nov.	29.749	40.633	29.764	40.633	42.133	1.70	11	19
Dec.	29.877	39.032	29.883	39.129	39.483	2.20	16	15
Aver. of year.	29.754	45.883	29.747	44.659	46.724	23.50	147	219

ANNUAL RESULTS.

MORNING.

BAROMETER.			THERMOMETER.		
Observations.	Wind.			Wind.	
Highest, Jan. 9	S W	30.88	June 26	W	68°
Lowest, Oct. 17	N W	28.58	Jan. 18	N W	1°

EVENING.

Highest, Jan. 8	S W	30.88	June 25	N W	67°
Lowest, Oct. 17	N W	28.66	Jan. 18	N W	9°

Weather.	Days.	Wind.	Times.
Fair	219	N and N E	19
Rain or snow	147	E and S E	97
	366	S and S W	67
		W and N W	183
			366

Extreme Cold and Heat, by Six's Thermometer.

Coldest, Jan. 18, Wind N W	one below Zero
Hottest, June 26, Wind N W	79°
Mean temperature for 1820.	46.7427

Result of two Rain Gauges.

	In. 100
Centre of the Kinfauns Garden, about 20 feet above the level of the sea	23.5
Kinfauns Castle, 129 feet	18.5

ARTICLE IX.

An Account of an Ascent to the Summit of Mont Blanc, in August, 1819. By Capt. J. Undrell, RN.

(To the Editor of the *Annals of Philosophy*.)

SIR,

London, March 23, 1821.

HAVING seen in your journal an account of an unsuccessful effort to reach the summit of Mont Blanc, perhaps you may give room for the detail of a prior attempt attended with better fortune. The letter I send you is a transcript of one written a short time after the ascent, and never, as you will perceive, intended for publication; but, as the relation may excite some interest from the melancholy event attending a subsequent failure, you may deem it worth a place in your publication, though possessing no requisite for a work solely dedicated to the purposes of science.

I am, Sir, your very humble servant,

J: UNDRELL.

DEAR —

Bern, Aug. 29, 1819.

You may feel curious to have the detail of my late ascent to Mont Blanc. I need not remind you that I had long intended making the attempt, but rather next year, upon my return from Italy, than at this period. In truth, when I left Geneva for Chamouny, on the 4th ult. the weather was so unfavourable that I scarcely expected even to see the summit of the mountain; and when the following day at Sallenche, I caught a glimpse of it at sun-rise, I deemed myself fortunate, as it became in a few minutes overcast. In the afternoon I reached the Prieuré, the rain falling in torrents, and although it cleared up a little the next morning, all the heights were still cloud-capt.

On the 7th, from that singular ice-bound is.e, called the Jardin, at the extremity of the magnificent glacier of Talèfre, which branches to the eastward from the upper part of the Mer de Glace, I had the first clear view of the top of Mont Blanc, which is thence beheld in a most imposing form, rising so abruptly for many thousand feet, that the snow cannot rest upon its sides.

On the 8th, the weather was unsettled; and on the 9th, I set off for Martigny, and had proceeded seven or eight miles towards the Tête Noire, when, turning to take a kind of farewell look at the mountain, then quite clear, as I was about to enter a path, which, from its direction, would have concealed it altogether; I all at once determined to return, and make an attempt the ensuing morning. My guide, Josef Marie Couttet, who had attended me in every excursion, was delighted at this resolution, and quite confident that the weather would prove favourable in spite

of what seemed to me strongly to indicate the contrary. The result justified his assumption.

In these regions there is no barometer equal to the local experience of the inhabitants, which may almost invariably be relied on. From auguring differently of appearances on quitting Geneva, I had, as stated before, postponed, in idea, the attempt till next year, and was thus imperfectly provided with instruments which cannot be got at Chamouny. Old Dr. Paccard supplied me with all he had, but I could not procure any good barometers, which I most wanted.

In addition to Couttet, I engaged five other guides,* who had all accompanied two American gentlemen a short time before, and left the Prieuré about five, a.m. on the 10th. We soon reached the base of the mountain, and after ascending, about an hour and a half, through a wood of firs and larches to the east of the Glacier des Bossons, reached a little cottage, the highest habitation, where we breakfasted. At half-past six, we departed, the thermometer being then 43° †; the sun shining bright upon the Dome du Gouté, and the western heights of the valley of Chamouny, which first receive his rays in consequence of the direction of the mountains. At twenty minutes past seven, we stopped at a large stone called Pierre Pointue, from whence the path to the "*dernier gazon*" is steep, winding, and difficult, having to the right the deep ravine by the side of the Bossons, which name is now applied to the whole extent of the glaciers towards the Aiguille du Midi and the Montagne de la Côte, including indeed a portion of what is properly that of Taconay. It was half-past eight before we reached the last land,‡ when we took a ladder, which is constantly kept there for these excursions, and, crossing a ravine covered with loose stones and huge fragments of rock, ascended the Bossons, over which congealed mass, the more lofty adjoining glacier, and the snows above them, our future path lay. Our shoes were fitted with the crampons used on such expeditions, without which it would have been impossible to have held footing; and we had also long staves

* These were Pierre Carrier, Alexis Du Vaussoix, Mathieu Balmaty, Eugene Couttet, cousin, and David Couttet, brother of the principal leader. I give their own method of spelling their names. All of these seem to have attended Dr. Du Hamel and his party; and the first, an excellent man, with two others, who had never before attempted the ascent, perished. The Doctor must be mistaken in asserting that poor Carrier had been 11 times at the top of Mont Blanc. He has confounded the unsuccessful efforts with those that succeeded, which are only about one in four. I should add that besides the above guides, there was a hardy youth of 18 who accompanied us, in order to become acquainted with the road.

† In taking the different temperatures, Reaumur's thermometer was used; in converting the degrees of which to those of Fahrenheit's scale, the fractional parts of a degree are omitted.

‡ In ascending, we saw several chamois at no great distance above us, leaping from rock to rock, with inconceivable agility. Their appearance quite agitated my guides. Their passion for following this poor animal is scarcely credible, and derives additional excitement from the dangers attending it. The animation of a fox-hunter is mere still-life compared with the *fever of delight* which pervades the chaser of the chamois.

pointed with iron, which are of infinite service in helping you onward, and a safeguard against falling.

There is no part of the ascent to Mont Blanc which I consider so perilous as that from the Glacier des Bossons to the rock called the Grand Mulet. Above, for a considerable space, are the prodigious masses of ice at the bottom of the Aiguille du Midi, rising to a vast height, and frequently overhanging their bases to such an extent as to menace with destruction every thing within their influence. Some of this fell shortly after we had passed, and an enormous quantity seemed likely to follow, and soon did, as will be shown by the sequel. At half-past 10, we rested upon a flat space of ice, or rather frozen snow, considerably higher than Mont Bréven, which is more than 8,300 feet above the sea, where the thermometer was 44° ; and at 15 minutes past 11, stopped under an immense quadrangular block of snow, pendant with icicles, and perforated in a most singular manner. The hollows were full of water, which we found very refreshing, as the heat was almost intolerable from incessant exertion. The thermometer, suspended in the sun, was 52° ; plunged, it sunk to 23° . The dangers increased after leaving this place, and called forth all the vigilance of the guides, accustomed from their infancy to cope with such difficulties. Though but a few weeks had elapsed since they were here, they found a vast change in appearances, and were obliged to proceed with the greatest circumspection. It is impossible to do justice to the courage and zeal of these people, and their devoted attachment to the persons employing them. As a single false step would have been fatal, we were linked together with ropes at 10 or 12 feet distance for mutual support. The route lay through masses of ice and snow, heaped confusedly together, and under every shape and form imaginable. Some of these were wholly ice, and so hard as to afford great resistance to the axe with which we were frequently obliged to cut out steps for our advance; others seemed in a state of half congelation; many were of snow quite soddened, of great relative gravity, and of a dark hue, from the moisture imbibed; and some appeared of a shining white, as if newly fallen, or having been lately precipitated from the upper regions of the mountain. We were obliged to pass sidelong over many of these huge heaps, inclined from 30° to 40° , often with a gulph beneath; and, occasionally, upwards, upon a mere thread of ice dividing the cavities, whose sides sometimes approaching seemed to unite at various distances below; while others, receding in their descent, or being quite perpendicular, presented nothing but a vast abyss whose termination was lost in darkness. Any thing thrown or falling by accident into these depths sufficiently marked their profundity. We derived great advantage from the ladder, for many of the chasms could not have been passed without it, even by the mountaineers who attended me. Though the distance from the

block of snow before mentioned to the Grand Mulet in a straight line is trifling, it took us three hours to attain it from the difficulties of the ascent, and the zigzag direction in which we were forced to move.

The Grand Mulet is a rock, or rather ridge of rocks, running towards the summit of Mont Blanc in a line about SSW and NNE. It lies between the chains of the Dromedaire and the Dome du Gouté, bearing S from the first, and SW from the second. The intermediate spaces form two valleys, but of unequal size, from the closer approximation of the Grand Mulet to the Dromedaire, and its continuation. The ascent from the Prieuré is from 70° to 72° , and from it to the top of the mountain about 60° . It has little appearance of vegetation; a few blades of common grass may be found growing in the angles of the crevices, and the surface is covered in many places with a lichen, of a bright-yellow colour. It would seem that nothing living would choose such a place for a residence, yet my guides assured me they had seen mice upon it, and this is confirmed by the fact, that, upon our return, one of them killed a weasel, which had no doubt, in some manner, made his way thither, to prey upon those animals.

The appearance of the snow towards the Dome and Aiguille du Gouté is singular and beautiful. We had passed over a chaotic mass of inequalities, but in that direction there is a striking order and regularity. The chasms run invariably nearly E and W, and the most extended are the lowest in the ranges. The line formed by the upper part of these rents is almost straight; that by the lower or detached part makes a curve pressing downwards. This form appears the consequence of the situation of the valley; for the ascending line being nearly N and S, the snow will naturally, from the effect of gravity, separate in an opposite direction. When the incumbent mass becomes too heavy to support itself, it breaks transversely, and thus makes those almost regular columns, as I should call them, or parallelopipeds, to which Saussure has given the name of Sêrac. The inclination and configuration of that part of the mountain upon which the snow rests, conduces to the formation of these blocks, and the operating causes being always the same, the effect of course will be so, and this succession of uniform masses must ever continue. The Sêrac holds its appearance for years, as the snows of several winters may be distinguished upon it, until it yields to the influence of circumstances, is resolved into smaller heaps, or, perhaps, descends in lavanges upon the valley below. The space between the Grand Mulet and the chain of the Dromedaire is wholly of these enormous parallelopipeds, but without any regularity of line. The ascent there is much more precipitous than on the other side, and the waters descending from the rocks, which on every side are almost perpendicular, produce more frequent and less uniform changes, and deprive the snow

of much of that dazzling whiteness which it usually wears at such elevations. The thermometer at our resting place at three, p.m. was 63° . It declined rapidly with the sun, and at half-past five was 43° . The space where we lay is on the SW side of the rock, and not more than 18 or 20 feet by 5 or 6. It has been made somewhat level by the guides, who have piled loose stones on the part towards the precipice as a protection. It was an unpleasant bed, but my guides slept as soundly as if lodged in the most comfortable mansion, and without being in any way disturbed by the thunder of the avalanches which kept falling, at short intervals, chiefly in the direction of the Mer de Glace. One of these men had lain 14, and another 11 times at the Grand Mulet, in various attempts to reach the top of Mont Blanc.

At half-past one next day (the 11th), we began our journey by moonlight. The ascent, though frequently steep, is not dangerous, having few chasms; so that we were enabled to advance without waiting for day-break. We were linked together as before, and made our way in the same order, the pauses of rest being delightfully filled up in contemplating a spectacle, to me so new and so magnificent. The sky at this elevation appeared before dawn almost black, the moon and stars seeming, as it were, sunk into the firmament. The snow-topped heights shone as if coped with silver—a broad expanse of clouds, undulated, but motionless and resplendent with the moon-beams, was stretched out beneath us, in the midst of which rose the bare dark pinnacles of the mountains, like rocks heaved up from the depths of ocean. The silence and solitude of the scene aided its effects, and induced a sensation which I might seek in vain for language to depict. At four, we reached the Petit Plateau, where the thermometer was 11° . The acclivity from hence to the more extensive level called the Grand Plateau is between 30° and 35° , the snow quite hard, and without many irregularities. As we advanced, the Dome du Goûté appeared magnificently above us; its snows formed in many parts into prodigious quadrangular masses, placed with uniformity, and bearing some resemblance to embrasures. Behind, we had the nearer range of the Savoy and Swiss mountains, brightening with the rising sun; and, far below, fringed with its exhalations, the long blue level of the Leman Lake.

At six, we reached the upper part of the Grand Plateau, whose height above the sea is fixed by Saussure at 12,710 feet. Here was the greatest degree of cold we experienced, the thermometer being 5° . All our provision was frozen, and the water we brought with us converted into ice. The sun had just beamed upon the place when we arrived, and during the hour we waited to rest and breakfast, the quicksilver rose nearly 16° . Here I first began to feel something of weariness and lassitude, accompanied with a want of appetite, and distaste for wine; and I

perceived that these sensations were participated, more or less, by all the guides. One of them, who had for a considerable time, with great labour, crawled along, was left at our departure quite exhausted, and asleep upon his knapsack. At seven, we set off, and the difficulties in a very short time far exceeded any that we had hitherto experienced. The route is to the left towards the Rocher Rouge, or east shoulder of the mountain, a more direct ascent being precluded by the formidable precipices of snow which lie between the summit and the Grand Plateau.* The inclination is in many places 50° , and the snow never hardens. The guides complained that since they had last been here, it had greatly accumulated, and some of them thought that all our trouble had been useless, but Couttet was quite certain of succeeding, though we sunk above our knees at every step, and were sometimes immersed so deeply that it was only by much exertion we could extricate ourselves. The wind, which blew freshly in our faces, increased the difficulty, and obliged us at every rest to turn quickly to leeward in order to inspire freely. I was obliged to limit every advance to 25 paces, and even this produced a painful throbbing, with great acceleration of pulse. It was during this march that I was surprised to see a butterfly upon the wing, and directing his course towards the top of the mountain. His flight, being against the wind, it was consequently the effect of choice. This was the only insect we saw alive, but several dead were picked off the snow. It was nearly 10 o'clock before we reached the Rocher Rouge, from whence, on the right, we had a full view of the majestic height which we were now certain of attaining. Here we rested a short time, and soon became quite refreshed. The day was remarkably fine, and the thermometer 41° . We now saw the guide who had been left asleep at the Grand Plateau, slowly making his way after us. Although he was not wanted, he would have deemed his credit lost had he not gained the top; and as it was, several of his companions expressed great contempt for the "imbecille," as they termed him, such being the spirit which animates these brave fellows.

The acclivity from the Rocher Rouge † is easy, not above 25° ,

* I cannot gather at what part of the ascent Dr. Du Hamel and his companions met with the deplorable calamity which ended so fatally to poor Carrier, and two other guides, as the account published in the *Annals of Philosophy* does not mention at what time the accident occurred. I should fancy it to have taken place shortly after quitting the Grand Plateau, and advancing towards the Rocher Rouge, as there are, or were, for the appearance of the snow is eternally varying, some deep chasms to the left in that direction, with many precipitous masses on the other hand which continue for a considerable space upwards. Portions of these heaps are frequently detached, and bear every thing before them, when set in motion from the slippery acclivity on which they rest. If my conjecture as to the place of the misfortune be well founded, Dr. Du Hamel must then have been more than three hours and a half from the summit of the mountain, which then seemed so near to him, so difficult is it to judge of distances at these elevations, and with such obstructions to encounter.

† I have preserved the appellations given by the guides, who have named almost

and without cavities ; but though it looked attainable with little trouble, scarcely any part of the journey was more painful. I had not before been very much incommoded by the rarity of the air ; but now I felt it so severely, and was obliged to make such frequent halts, that it was almost an hour and a half, although the distance is trifling, before we conquered it. The inconvenience seemed chiefly attendant upon progressive motion ; for mere exercise, without change of place, did not much affect me. The surface of the snow toward the top is completely waved, like water ruffled by a slight breeze, or rather, perhaps, like a heap of sand furrowed by frequent showers ; yet it is the opinion of the Chamounese that it never rains upon Mont Blanc, as they observe, whenever this takes place below, that the highest rock visible to them is covered with snow. At so many thousand feet above the curve of congelation, it cannot indeed be otherwise ; yet a cursory glance would almost induce one to think the contrary, as, independent of the appearances before mentioned, a portion of the ascent was strewed with thin shining plates of ice, like water arrested in its passage by the hand of frost. It was half-past 11, a. m. exactly 10 hours after leaving the Grand Mulet, that I had the pleasure to find myself upon the summit of the ancient continent. The highest part of Mont Blanc has been said to resemble in form an ass's back. It is a narrow and almost level line of little extent, running nearly E and W, and somewhat higher at the western end, curving gradually towards the N to the vertical snows above the Grand Plateau ; and in the same way to the S, to the prodigious precipices over the valley of Entreves. The prospect from this colossal height was, as may be expected, immense, and only bounded by the imperfection of human vision. The weather was beautiful, the air quite clear, and the wind between the S and W, which is what generally produces or accompanies those optical illusions so frequent in the Alps, when distance seems annihilated, and objects the most removed appear, as it were, within the grasp. To the E the eye stretched over the Milanese ; to the SE, to the Parmesan and adjacent countries ; and S, towards Genoa, and, perhaps, to the Mediterranean ; but of this last, I cannot speak with certainty. The Apennines intercept the view ; and I might be, and probably was, deceived in what I took for that sea.* A large portion of the south of

every portion of rock the snow has not covered. A ridge near the Rocher Rouge, they term the Petit Mulet. The east shoulder is properly Mont Maudit.

* In Ebell's "*Manuel du Voyageur en Suisse*," it is stated that Mons. Bourritt, with his son and three guides, reached the summit of Mont Blanc in 1788, "*malgré la grêle*," which had dispersed the rest of the party ; and that, descending for shelter to the SE, Mons. B. had from thence fancied he saw the Mediterranean. The ascending Mont Blanc in such weather as described, I conceive to be physically impossible, and useless, from obvious reasons, were it otherwise. In fact, it is denied at Chamouny that Mons. B. ever was at the top of Mont Blanc, or at any part of it, whence, from the form of the mountain, the Mediterranean could be visible. The whole history is much laughed at by Dr. Paccard, and the old guides of Chamouny.

France was distinctly visible, with the most distant range of the Swiss mountains, and beneath, in a manner under our feet, the Mer de Glace with its glaciers, and the vale of Chamouny, whose inhabitants were clearly seen through a telescope. The Alps had a very singular appearance. The height from which they were beheld seemed to rob them of their character as mountains; and the relative elevations of the greater number were hardly perceptible. Except Monte Rosa, which towered with nearly rival grandeur, and a few others, the whole looked like a vast expansion bristled with rugged inequalities, whose tops almost invariably ended in a point. These pinnacles ran in various directions, but in lines parallel to each other, forming between them ranges of hollows covered with snow. This needle-like termination appeared universal, as except that upon which I stood, and Monte Rosa, I scarcely perceived an exception in the wide extent of Alpine region, which lay under my view. The clouds were all far beneath, and quite at rest. Many were below the mountain-summits; some upon them, and others, altogether detached. Their forms were wholly semiglobular, or rather a continuation of segments of circles, the extremities of which were defined with perfect exactness. A few that were nearly separated from the larger bodies were completely round, and seemed like small globes resting upon one of their poles. These clouds were all nearly opaque, and appeared of a dusky white, luminous at the edges; but, just before I left the top of the mountain, when the sun had considerably declined, some of the more distant, particularly towards the east, began to assume that bright empurpled rose-hue tint, so frequently attendant upon, and I believe peculiar to, an Alpine sun-set. The sky was of a dark, deep blue, many shades more intense than when viewed from below. This colour, which approached to black at the zenith, diminished gradually to the horizon, where it had a kind of violet tinge. Nothing can be fancied so beautiful as the ethereal concave, arching out, if I may so express it, into infinity, without any exhalation or impurity of earth to intercept its magnificence. There was a something in the scene and the situation—a feeling of high-wrought enthusiasm, to which the mind willingly lent itself, seeming to stretch beyond the wonders it contemplated towards Him who formed them.

I remained upon the top of Mont Blanc three hours and a half. During this time, the alterations of temperature were continual. The wind in general blew strongly from the SW, but, at intervals, it died away, and a milder atmosphere was the immediate consequence.

When we first arrived, the thermometer in the sun was 33° ; placed upon the snow with the same aspect, it sunk in five minutes to 25° . Suspended towards the N in a cold current of air, it was 14° ; and in the same spot, with the bulb in the snow, fell to 9° . All these changes took place before mid-day. The

greatest degree of heat we experienced, or rather seemed to experience, for it was not indicated by the thermometer, was about two, p.m. The glare of the sun, from which we had no direct shelter, and the reaction of his rays becoming more painful from prolonged exposure to them, must have caused this sensation. The wind afterwards increased, and when I quitted the summit, the thermometer in the sun and full air was 19° . This is a much greater degree of cold, and a more extended variation of temperature, than was felt by Saussure, though his ascent was in the same month of the year, and only a few days earlier—a proof of the changes prevalent at such elevations. That eminent philosopher mentions that by placing himself in the shade, he was able to see the stars; but though I lay down in the snow, and had my guides with their cloaks, &c. spread about, so as to form a kind of night around me, I was not equally fortunate, which may, perhaps, be attributable to the difference of wind and atmosphere. Sound is imperfectly conveyed at great heights, and it is not in the slightest degree reverberated upon Mont Blanc. I caused a pistol to be discharged several times at various distances; but the cessation of the report was instantaneous, and when I had it fired into a cavity in the snow, and lay down 30 or 40 paces off, the effect was the same.

While upon the summit, I sent my guides to break off some of the highest visible rock* in Europe, which is considerably lower towards the SE; and I send you specimens, with some stone vitrified by lightning, which is occasionally found in that direction. This is the only effect of that element discernible upon Mont Blanc, which, by its form, and the snowy cope with which it is eternally crowned, is shielded from its influence. At three o'clock, having assembled my guides, and caused them to join with me in drinking to the prosperity of Old England, we commenced our descent; and in an hour and a half reached the Grand Plateau. We were linked in ascending, and passed the declivities, which were not too precipitous, by gliding down them upon our haunches—a method attended with no other inconvenience, except that the foremost in the train were covered with the rubbish raised by the passage of those behind. In this manner we proceeded, and arrived at the Grand Mulet in three hours and twenty minutes. We found that two avalanches had fallen in different parts of our path, neither of them indeed very large, but quite sufficient to have overwhelmed the whole party.

* Dr. Du Hamel is mistaken in supposing that he had procured pieces of the “highest visible rock in Europe,” that being situated only about 250 feet below the top of the mountain, but several thousand above the spot, whence he seems to have got his specimens. This rock is the protogene of Prof. Jurine, but the Rocher Rouge is granite, and has a reddish hue, whence its name.

While going along, I could not help fancying that a large portion, or the whole, of the stupendous compact body of snow from the Grand Plateau downwards might be continually and progressively descending. The disposition of the mass, and the situation of the chasms always lying where the inclination is considerable, with the general appearance of the whole, suggested the idea. This is, however, but a mere conjecture, and even were it otherwise, it is not ascertainable, as a long series of years, perhaps ages, must pass by before the part now uppermost could find its way into the plains. The cold at the Grand Mulet was very severe during the night, and we were very poorly skreened from it by the slight covering which, from the rock on one side to the precipice on the other, we had hung obliquely over us.

At five, a.m. on the 12th, we set off for Chamouny, the path being too dangerous to make the attempt till perfect day-light. Our passage was impeded by a tremendous avalanche, which had fallen upon the spot, which, as I stated, it seemed to threaten as we ascended from the glaciers at the foot of the Aiguille du Midi. It covered an extent of more than a quarter of a mile, and we were full half an hour in traversing it. It was nine o'clock before we reached the "dernier gazon." My eyes had now become much inflamed,* as I had used little or no precaution to defend them against the reaction of the sun from the glare of white around me, the scene being too extraordinary to be viewed even through the preservative of green crape, or any other medium, and I scarcely recollect any thing more painful than the first flash of the sun upon them as we descended. At half-past 10, we stopped at the Chalet, where we had refreshed ourselves two days before; and here the thermometer was 65°. At half-past 11, we reached the bottom of the mountain, and at noon arrived at the Prieuré. The heat in the valley appeared intolerable to us, though only 72°.

On the morning of the 16th, the affection in my eyes having ceased, I crossed, with my old guide Couttet, over the Tête Noire to Martigny, and thence by Bex, Chillon, Vevay, &c. to Lausanne, where I arranged my notes, and sent to ——— a little account, which you will here have more at length. I fear, however, that you will fancy it unreasonably extended, and wonder why any one should take so much pains to so little purpose, and without any scientific result; nevertheless, as the attempt succeeded, I consider myself amply compensated for all the dangers attending it; for I wished to contemplate Nature in her mightiest works—to behold the clouds from a region above

* Having accidentally met Capt. U. shortly after his descent, and witnessed what he has stated, I cannot too strongly recommend the adoption of those precautions on similar occasions which he neglected to observe.—*Edit.*

them—and to see, if such a phrase may be permitted, how Heaven looked from one of the pinnacles of earth.

* * * * *

Ever sincerely yours,
J. UNDERL.

ARTICLE X.

Further Observations on the Comparative Advantages of illuminating by Gas produced from Oil and from Coal. By M. Ricardo, Esq.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Bow, March 21, 1821.

I AM unwilling to intrude the subject of oil gas again upon your readers; but there are some observations made upon my former paper by a correspondent in the last number of the *Annals*, to which I must claim permission to reply. This gentleman, whoever he may be, has said more (unintentionally, no doubt) against the management of coal gas companies in general, than could be deduced from any thing I have written. My object was only to prove the inferiority of coal gas, when brought in comparison with that of oil; but he has shown that some great mismanagement exists in all the companies, excepting those of Sheffield and Derby; and if the statement respecting the latter be correct, the management of the Sheffield Company may also come in for its share of blame.

I must still persist in my assertion, notwithstanding your correspondent differs from me, that the advantages attendant on the London Chartered Company, are greater than any other possess. It must be allowed, that to have the greatest number of lights in the smallest compass is the highest advantage which a gas light establishment can have. The cheapness of coal and labour is of little benefit, unless there is an ample demand for the gas; and what place is likely to compete with London in this respect? If you compare this Company's main with the number of lights upon it, I should think no provincial city or town could in any way equal it.

Your correspondent says, that the selling price of coal gas at Sheffield is upon the average 10s. 6d. per 1000 cube feet, and at this price, it yields a profit of 10 per cent. He then asks somewhat triumphantly, what I shall say, when informed that at Derby, gas is sold at 7s. 6d. per 1000 cube feet, and that the Company "calculates" to share a profit of 10 per cent.? To this

I can only reply, that without doubting your correspondent's veracity, I must beg leave to doubt the fact till the Company's calculation is realised; besides, if Sheffield retails its gas at 10s. 6d. with a profit of only 10 per cent. there must be some extraordinary and unaccountable advantages attendant on Derby, or some gross mismanagement on Sheffield, as I cannot see why the latter should not be able to sell their gas as low as the former, and at that rate the profits ought to be nearer 60 per cent. than 10 per cent. From a paper now before me, the Derby Company appears to be but very recently established, and the promoters of it are making their calculations by anticipation. One circumstance I beg to mention, where I conceive them to be completely in error; and if their calculations of profit are made from such data, they will certainly be deceived. This paper states, that 1000 cube feet of coal gas are equal to 70 lbs. of candles. This, I believe, is at variance with every recorded experiment that has been tried, and more particularly with those of Mr. Accum, one of the chief promoters and warmest advocates of coal gas establishments. In my former paper, I have given the result of his trials on this subject, and, according to them, supposing these experiments to have been made with candles six to the pound, instead of eight, each candle lasting six hours, a pound would be equal to 24 cube feet of coal gas, so that 1000 cube feet would be equal to somewhat less than 42 lbs. instead of 70 lbs. as stated in the Derby report.

Your correspondent next draws a comparison between my calculation of the expences of oil gas and the Sheffield coal gas; but I cannot admit that one of the former is equal only to three of the latter: I am convinced it is fully equal to four, in drawing the medium between the two; and allowing it to be as one to three and a half, I trust I have made every necessary allowance. In that case 1000 cube feet of oil gas which cost 30s. would sell for the same price as 3,500 of coal gas, instead of 3000; that is, 1*l.* 16*s.* 9*d.* yielding a profit of 22 per cent. instead of 10 per cent. as stated by your correspondent.

I am willing to allow that some of these calculations show better on paper than in reality, and that there are always some great and unavoidable expenses which cannot be immediately foreseen, but which, however, it becomes necessary for a newly established Company to guard against by a high price. This may always be much more easily lowered than raised; but whatever these contingencies may be, they must, from the nature of the two establishments, be greater in the coal than in the oil. In estimating the profits, it must always be done with the consideration of a certain demand for a given number of lights: interest on capital, and expences of management to a certain extent, must be incurred, whether there are few or many lights required; but when these are properly balanced by the demand, a profit approximating to what I have stated may be expected.

Your correspondent says, that the more unpleasant the gas is, the more readily is its escape detected. I am very willing to allow, that though not so unpleasant as coal gas, oil gas is quite enough so to insure detection. The only question in dispute then is, which may be procured the cheapest. I think I have proved that the advantages rest with oil gas; but even supposing it were as expensive as coal gas, its superiority in every other respect will, when its use is as well known, give it a decided preference. I have subjoined a table of the cost of gas at 12 different establishments. This has been kindly furnished me by a friend, who undertook, from their printed rate cards, to make the necessary calculations, and which, I believe, may be strictly depended on for their accuracy. This table will be a sufficient proof that in estimating the selling price of gas generally at 15s. per 1000 cubic feet, I have not been so wrong as your correspondent has assumed; but, on the contrary, that my estimate has been too low instead of too high. The average price for the 12 establishments quoted is 16s. 10d. instead of 15s. I have been unsuccessful in my endeavours to procure rate tables either of the Sheffield or Derby companies. I have seen a printed paper from Derby which states the *intention* of the promoters of that establishment to sell gas at 7s. 6d. per 1000 cubic feet; but I have seen no document from which it can be inferred that they actually do so sell it.

Were I to hazard an opinion on the cause of such small profits as are generally known to accrue even from gas establishments which make high charges, I should attribute it to the injudicious mode of charging for light; namely, by the number of burners used instead of by the quantity of gas consumed; in the former way, there is no check upon the consumer; and every one who walks the streets in the evening must observe the extravagant waste of gas in the different shops—an extravagance which does not benefit the consumer, while it very seriously injures the Company providing the gas. This might be obviated by the use of the gas meter: the perfection to which this machine is now brought, and the correctness with which it registers the quantity of gas that passes through it, will ensure its preference over every mode of estimating the price of gas both to the Companies, and the consumers, if it became generally adopted.

Table of the Prices at which Gas is charged per 1000 Feet, calculated from the Rates of various Places; taking the large Argand Burner until 10 o'Clock as the Standard, and reckoning the Consumption of Gas at five Cubic Feet per Hour for such Burner, and the Number of Hours in one Year to be 1100, or $3\frac{1}{2}$, on the Average for 313 Days.

	Annual charge.			Cube feet consumed in a year.	Rate per 1000 cube feet.		
	£	s.	d.		£	s.	d.
Leeds	4	0	0	5500	0	14	6
Edinburgh	4	4	0		0	15	3
Newcastle	4	4	0		0	16	4
Bath	4	10	0		0	16	8
Worcester	4	12	0		0	17	2
Chorley	4	14	6		0	17	5
Preston	4	14	6		0	17	5
London	4	16	0		0	17	5
Liverpool	4	16	0		0	17	5
Bristol	4	16	0		0	17	5
Chester	4	16	0		1	0	4
Oxford	5	12	0		1	0	4

The average charge for 1000 cube feet of the 12 towns is 16s. 10d.

P.S.—Since writing the above, I have been successful in obtaining a Sheffield rate card. Calculating on the same scale as the others have been done, I find the cost of gas is 17s. per 1000 cube feet instead of 12s. as stated by your correspondent. The highest discount which they allowed is 20 per cent. on large payments: this, taking the medium, would average the gas at 15s. 2d. instead of 10s. 6d.: the quantity sold by the meter at any of the works is so small, I understand, that no calculation can be made from that mode of charging. The difficulty which my friends have had in procuring me the rate cards has made me so late in transmitting this paper to you; and this last, of Sheffield, the most important of all, I have only obtained this morning. I am aware that the time of your receiving communications has elapsed; I cannot any longer encroach upon your indulgence; and instead of rewriting the paper altogether, which I should not be able to do for this month's insertion, I must request you will insert this as a postscript. Although I entertained some very considerable doubts of your correspondent's correctness, I did not like to express them till I had satisfactory proofs instead of mere surmise; these are furnished by the rate table before me; and the charge which your correspondent

brought against me of not obtaining better information before I estimated the price of gas at what I did in my former paper, may certainly be retorted upon him, for not having made his statements more correct than he has done. I am, however, indebted to him for affording me an opportunity, not only of confirming my former statement, but of showing that, instead of exaggerating the superiority of oil gas in point of economy, I have underrated it, and that its advantages are much greater than I had myself conceived before I entered into this more complete investigation. Had it been practicable, it would not have been uninteresting to have shown what dividends are paid by the above enumerated Companies; how many pay 10s. per cent.; how many pay more; how many pay less; and how many pay none at all.

I am, yours, &c.

M. RICARDO.

March 23, 1821.

ARTICLE XI.

Remarks on Capt. Kater's Experiments on the Length of the Pendulum.

(To the Editor of the *Annals of Philosophy*.)

SIR,

As notices of Capt. Kater's experiments on the length of the pendulum have occasionally appeared in your journal, the following remarks relative to that subject may, perhaps, be allowed a place in it. I am, &c. &c. X.

An article appeared in the *Edinburgh Review* for November, 1820, giving an account of Capt. Kater's experiments for determining the length of the pendulum at different stations in Great Britain; and the writer has made some observations which seem to be founded in an erroneous view of some of Capt. Kater's statements. At p. 343, the following passage occurs:

"Capt. Kater seems to have mistaken the import of Dr. Young's statement, when he uses this correction for the attraction of the 'elevated part interposed between the general surface and the place of observation,' nothing being said of lateral attraction caused by surrounding matter. But Capt. K. applies the correction for the error produced by hills lying round the point of observation."

On this passage I have to remark, that Capt. Kater does not apply the correction for the error produced by hills lying round the point of observation. Indeed the only proof which this writer brings to support his charge is, that Capt. K. says, "the

388 *On Capt. Kater's Experiments on the Pendulum.* [MAY, height of the station at Unst was found to be 28 feet above low water; whence we have 0.12 for the correction, as deduced from the squares of the distances from the earth's centre; and as the station at Unst was *surrounded* by hills composed of serpentine, I shall take $0.12 \times \frac{1}{4} = 0.06$ for the correction to be applied in order to obtain the number of vibrations which would be made at the level of the sea.—(Phil. Trans. 1819, Part III. p. 354.)

Here it is surely obvious that he introduces the consideration of the nature of the hills *surrounding* his station, not for the purpose of applying a correction for *their* attraction, but as affording a presumption of what might be the nature of the substratum of the place of observation;—a consideration which Dr. Young's rule requires to be introduced, as the correction varies not only with the nature of the eminence, but also with its density.

Having thus attempted to make out that Capt. K. has *mistaken* and *misapplied* Dr. Y.'s rule, this writer next proceeds to find fault with the *quantity* deduced from the rule. After quoting Dr. Y.'s account of his correction from the Phil. Trans. 1819, Part I. p. 93, and stating that the correction for a tract of table land of a mile in thickness will be $\frac{1}{1000}$ of the whole correction for elevation; he proceeds to remark, "If this be the case, we cannot perceive the grounds on which Capt. K. takes $\frac{1}{4}$, only a little less than $\frac{1}{1000}$ for the correction applicable to an elevation of 28 feet, in the actual state of the superficial inequalities. We may have overlooked some step in his reasoning, or Dr. Y.'s; but we feel bound to state our difficulty as it occurs."

There is doubtless some difficulty in the case; but from Dr. Y.'s own words, it appears that he considers "that for a place situated on an elevated table land of a mile in thickness, of the *mean density* of the earth, the allowance for elevation would be reduced to one-half; and in almost any country that could be chosen for the experiments, it must remain less than three-fourths of the whole correction deduced immediately from the duplicate proportion of the distances from the earth's centre." Thus the correction (as Capt. K. remarks) will vary according to the nature of the elevation, and also its density from one-half to three-fourths of the quantity before deduced. In *any* situation it will not be reduced less than one-half; nor more than three-fourths. These considerations seem to have been overlooked by this writer; still, however, I do not mean to assert, that the numbers are absolutely correct. It is to be wished that both Capt. K. and Dr. Y. had given a more detailed account of the method of finding this correction.

In a subsequent page (347), this writer observes; "from the great disturbing force at Arbury Hill, we may infer that there exists very near it a mass of matter of considerable density. Capt. K. conjectures that this mass is Mount Sorrel, which consists of granite, and other rocks of primitive formation are situated in its vicinity."

Now here it is to be observed, that Capt. K.'s conjecture is misrepresented, as will be clear from a reference to his own words, *Phil. Trans.* 1819, Part III. p. 425:

"It would be no improbable conjecture, that the sudden increase of gravitation, observed at Arbury Hill, may be occasioned by a rock of primitive formation *approaching the surface of the earth in the vicinity of that station.*"

He then subjoins in a note, "I find the conjecture I have hazarded remarkably supported by fact; for on consulting Smith's Geological Map of England, it appears that Mount Sorrel, a mass of granite, is situated, together with other rocks of primitive formation, about 30 miles to the N of Arbury Hill."

This is surely very different from saying that the attracting mass is Mount Sorrel. That rock is obviously mentioned only to show the probability that granite may form the *substratum* of some point near the place of observation.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 5.—A paper was read, "On the Separation of Iron from other Metals," by J. F. W. Herschel, Esq.

Mr. Herschel proposes, as the basis of a rigorous separation of iron from the metals not precipitated by sulphuretted hydrogen which it most usually contaminates (manganese, cerium, nickel, and cobalt), a peculiarity in the peroxide of iron, in virtue of which it is incapable of subsisting in a neutral solution at the boiling temperature. If a solution of this peroxide be neutralized when cold, and then heated, a portion is deposited in the state of a subsalt, and the liquid becomes acid. If allowed to cool, and again neutralized, a fresh portion of the metallic contents separates on re-applying the heat, and so on, till the quantity held in solution is no longer sensible to the most delicate reagents. If, on the other hand, the neutralization be performed *while actually boiling*, we attain this limit at one operation. Hence Mr. Herschel recommends the following process: Having peroxidized, by means of nitric acid, a solution containing iron and any of the above-mentioned metals, drop into it, *while boiling*, carbonate of ammonia, till the acid reaction is entirely destroyed, and *even going a little beyond the point of exact neutralization*. The whole of the iron to the last atom is separated, while the liquid retains in solution the other metallic oxides, *as well as the minute portion of their carbonates due to a*

trifling excess of the alkaline precipitant: In the cases of cobalt and cerium, the alkaline carbonate may be added in considerable excess without separating any of those metals, and their solution so freed from iron is then a most delicate test of the presence of the latter metal.

April 12.—A paper was read, On the Mean Density of the Earth, by Dr. C. Hutton.

At the same meeting a paper was read, On the Restoration of a Portion of the Urethra in the Perinæum, by H. Earle, Esq.

GEOLOGICAL SOCIETY.

Feb. 16.—An introductory essay on the Geology of India, by H. T. Colebrooke, Esq. MGS. was read.

The physical geology of India may be considered as resolving itself into three great divisions; viz. 1. The peninsular tract, which constitutes the south of India; 2. The belt of flat country extending from sea to sea, and distinguished by the name of Middle India; 3. The continental mountains which form the northern limit of India, rising between the middle region and the vast extent of Tartary, and extending more than 15° of longitude in a direction from WNW to ESE.

In the champaign country constituting middle India, three principal divisions may be noticed; 1. The tract watered by the Ganges and its tributary streams; 2. A Tract watered by the Indus; and, 3. The intermediate desert, on which the Saraswaté loses itself.

Of this country, a striking feature is the total absence of pebble or rolled stones of any kind, except in the beds of the rivers, for a few miles after they quit the hills; and the subsoil of the plain is every where earthy and comminuted, except in certain instances, where nodules, or concretions, have been found.

Throughout this extensive plain, there is neither mine nor quarry: the banks of the river being usually precipitous on one side, and shelving on the other, exhibit sections of strata down to the level of their beds. Scarcely any other natural section is found; and the sinking of wells, or boring for water, is the only opportunity which art presents for the examination of strata.

The surface is every where alluvial, and the strata, as far as they have been observed, are horizontal. Beneath the superficial mould the subsoil is sand, clay, or loam in layers more or less intermixed, and distinguished by colour or texture. In the inferior strata of clay, nodules or concretions of the same substance are sometimes met with. The upper strata of siliceous sand, as well as that found in the bed of the Ganges, generally abound in fragments of mica; but in some places, beds of sand contaminated with salt, and in others, beds exclusively composed of salt, are found.

In a very few spots, and at no great depth below the surface,

nodules of a calcareous nature and irregular shape are met with, which, on calcination, afford an impure lime; but throughout the low country, limestone is generally deficient. A small hill at Membari, in North Bengal, being one of the few instances of detached hills in the midst of this champaign country, is a rock composed of rounded pebbles and angular nodules imbedded in a cement of like nature, but different colour. Both effervesce with acids, and the cement leaves the larger insoluble proportion.

In some places, at a considerable depth below the surface; for instance, at Calcutta, at the depth of 30 to 35 feet, fossil wood not petrified, but more or less rotten and decayed, is found, and sometimes in large blocks. Vegetable petrifications are also sometimes met with, and in particular silicified wood.

Except fragments of shells abounding in the fluviatile sand, no animal exuviae have as yet been found within the limits of the low country of Middle India.

March 2.—Part of an "Outline of the Geology of Russia," by the Hon. W. I. H. F. Strangways, MGS. was read.

Of the Russian empire, the two great divisions, viz. Russia, properly so called, and Siberia, must be considered in a geological point of view, as perfectly independent of each other, the same boundary dividing the two countries, and the two tracts of secondary formation belonging to them. The empire contains five principal mining districts; viz. two in Europe, two in Asia, and one on the confines of Russia and Siberia. Those in European Russia are the northern or Finnish district, and the central; the former reaching from the gulf of Bothnia to the lake Onega; the latter stretching across the country, in an oblique direction, from the government of Kulouga to that of Nishegorod. The border, or Oural district, comprehends the Oural mountains as far as they have been explored. The two mining districts that lie entirely within the frontier of Siberia are those of Kolyvan, on the west frontier of China, and Nerchinsk, on the frontiers of China and Siberia towards the Pacific Ocean.

In traversing Russia from north to south, we find a great extent of primitive country comprehending Russia, Lapland, Old and New Finland, the northern parts of Carrelia, and part of the government of Olonetz, and forming evidently a prolongation of that of Sweden, with which its connection may be traced by the Isles of Aland, on the south by those in the centre of the Gulf of Bothnia, and by the Lapland chain of mountains on the north. Of this district, the northern parts are said to consist principally of trap rocks, the central of gneiss, and other varieties of schistose rocks; while the northern border is composed entirely of granite.

The islands of Pargas in the Gulf of Bothnia, about two miles south of Abo, present in general the same features as the main land, being in fact but continuations of the hills of the continent.

On one of the principal islands of this group, the mineral, known by the name of pargasite, is found in one or more large veins of white primitive limestone which traverse the island from side to side, and which seem to bear some analogy with respect to their geological situation, and their external characters, to the limestone of the Hebrides, especially in the Isle of Tirey. The country through which this limestone passes is gneiss, the fissures of which are in a direction parallel to the course of the vein. The breadth of the vein varies from 20 to 100 feet.

ASTRONOMICAL SOCIETY.

April 13.—A paper, from Mons. Nicollet, of the Royal Observatory at Paris (communicated by the Foreign Secretary), was read this evening: it contains his own calculations of the elements of the comet lately discovered by him in the Constellation Pegasus; they are as follows, viz.

Perihelion passage, March 21 9° 33' 7" Paris time
 Perihelion distance. 0.091113
 Log. Perihel. dist. 8.9595327
 Longitude of ascending node. 48° 32' 12"
 Longitude of perihelion (on the orbit) 239 18 37
 Inclination of the orbit 74 10 53

Motion retrograde.

A paper likewise on the same subject, transmitted to the Society by Dr. Olbers, one of its associates, was read; it contained the following elements calculated by Professor Encke, of Seeberg, by Professor Nicolai, of Manheim, and by Mons. Von Staudt, of Gottingen.

	Prof. Encke.	Prof. Nicolai.	M. Von Staudt.
Perihel. passage ..	Mar. 21. 405	Mar. 21. 6016	Mar. 21. 6026
	M.T. Seeberg.	M.T. Manheim	M.T. Gotting.
Long. of perihel. . .	239° 20' 45"	239° 34' 5"	239° 36' 0"
Log. perihel. dist. .	8.95966	8.96466	8.9641627
Long. ascend. node	48 34 37	48 43 34	48 45 44
Inclination of orbit.	74 5 0	73 23 15	73 16 33

Motion retrograde.

In this paper also allusion is made to the anomalous appearance lately observed on the moon's disc, of which it will be remembered that an account was presented to the Royal Society by our countryman Capt. Kater. The Doctor, however, differs from the British philosopher as to the cause of the phenomenon, and does not consider it volcanic. †

A Description of an improved Repeating Instrument, by George Dollond, FRS. and member of this Society, was then

read. Its advantages over others are obtained by applying a transverse axis to the telescope and declination circle, thereby giving them all the perfections of the transit instrument, without diminishing their repeating properties; and by the application of two levels alternately becoming finders to the telescope, which render the back semicircle unnecessary, a contrivance which saves much loss of time when repeating zenith distances. The instrument is also constructed to repeat in azimuth, and is furnished with a very delicate level, as a check upon the horizontal circle, and which answers all the purposes of an under telescope. The object glass of the telescope is of two inches diameter, and 17 inches focal length: with it the pole star may be distinctly observed in the day time. The angles are read off to every tenth second by verniers and fixed microscopes. The instrument differs from all others previously made; is furnished with every requisite for ascertaining vertical and horizontal angles, and which, if taken singly, may be, in the author's opinion, as correctly obtained by this, as by any instrument of the same dimensions not possessing the repeating principle.

ARTICLE XIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. Purple Powder of Cassius.

Dr. Clarke, of Cambridge, has recently examined the purple powder of Cassius; and from his experiments, he concludes, that the binary compound which he analysed consisted of the oxides of tin and gold, and contained these oxides chemically combined in the exact proportion of three parts of tin to one of gold; and that the alloy of the two metals obtained by the fusion of 100 parts of the purple powder would yield

Metallic tin.....	75
Metallic gold.....	25
	<hr/>
	100

Because 8-10ths of a grain of the alloy yielded

Metallic tin.....	0.6
Metallic gold.....	0.2
	<hr/>
	0.8

Dr. Clarke also infers, that in precipitating the purple powder of Cassius from the muriate of gold, by means of the muriate of tin, the two metals, tin and gold, are thrown down as oxides, which, however, do not chemically combine in a constant relative proportion to each other; that the quantity of tin always exceeds that of the gold; and that

the difference observable in the hues of the precipitate made at different times is to be ascribed to the different proportions in which the oxides of the two metals have combined together, and, perhaps, also to their different degrees of oxidation.—(Transactions of the Cambridge Philosophical Society.)

II. Quantity of Copper raised in Cornwall.

In Six Months ending June 1819.

The produce of 58 mines, 3327 tons; of which, 1736 tons were produced by six mines as under:

	Ore.	Copper.
United mines	5005 tons	419½ tons
W. Abraham, &c.	5496	395
Dolcoath	470½	345½
Treskerby	2313	197
Wheal Squire	2353	195½
Crennis	2407	183½
	<hr/> 22278	<hr/> 1736

Produce of the ore about $7\frac{1}{2}$ per cent.

Price of copper, 131½. per ton.

In Six Months ending Dec. 1819.

The produce of 74 mines, 3477 tons; of which, 1768 tons were produced by six mines as under:

	Ore.	Copper.
United mines	5817 tons	482 tons
W. Abraham	5655	395
Dolcoath	5305	375
Wheal Squire	2053	188
Treskerby	2170	185
Wheal Unity, &c.	1873	148
	<hr/> 22873	<hr/> 1768

Produce of the ore about $7\frac{1}{2}$ per cent.

Price of copper 124½. per ton.

In Six Months ending June 1820.

The produce of 66 mines, 3545 tons; of which, 1760 tons were produced by six mines as under:

	Ore.	Copper.
United mines	5798 tons	511 tons
Dolcoath	5438	406
W. Abraham, &c.	5298	364
Treskerby	2206	223
Wheal Unity, &c.	1693	130
Wheal Squire	1415	126
	<hr/> 21848	<hr/> 1760

Produce of the ore about 8 per cent.

Price of copper, 114½. per ton.

In Six Months ending Dec. 1820.

The produce of 72 mines 3962 tons, of which, 1891 tons were produced by six mines as under :

	Ore.	Copper.
United mines.	5594 tons	481 tons.
Dolcoath	5665	408
Wheal Abraham, &c. ..	5069	388
Treskerby.....	2199	213
Wheal Drewollas.....	2074	205
Pembroke.....	2612	201
	<hr/> 23213	<hr/> 1891

Produce of the ore about 8½ per cent.

Price of copper, 113*l.* 10*s.* per ton.

General Return of Copper raised in England and Ireland.

One Year ending June 1819.

	Tons.	Cwt.	Qrs.
Cornwall.....	6974	2	2
Anglesea.....	564	0	0
Devon.....	433	0	0
Ireland, Ecton, and other mines in Staffordshire and elsewhere*	596	10	8
	<hr/> 8567	<hr/> 13	<hr/> 1

One Year ending June 1820.

	Tons.	Cwt.	Qrs.
Cornwall.....	6915	4	1
Anglesea.....	561	0	0
Devon.....	414	0	0
Ireland, Ecton, and other mines in Staffordshire and elsewhere*	813	11	1
	<hr/> 8703	<hr/> 15	<hr/> 2

III. Native Hydrate of Magnesia.

The native hydrate of magnesia was first discovered, and ranked as a separate mineral, by the late Dr. Bruce, of New York. It was found only at Hoboken, in New Jersey, traversing serpentine in all directions in veins of from a few lines to two inches in thickness.

Dr. Hibbert found this substance in 1817 at Swinansess, in Unst, one of the Shetland Isles, traversing serpentine in all directions, being mixed with the magnesian carbonate of lime, and forming veins from half an inch to six or eight inches broad.

Chemical Character.—Hydrate of magnesia dissolves entirely in

* Part of this quantity is from Cornwall; but from the mode of sale, no exact division can be made.

muriatic, nitric, and dilute sulphuric acids; and from its solution in muriatic and sulphuric acids, the deliquescent salt of muriate of magnesia and regular crystals of sulphate of magnesia were obtained. On some occasions, a very slight effervescence takes place; but this no doubt arises from adhering particles of carbonate of lime, or from a small quantity of carbonic acid, which may have been absorbed by exposure to the atmosphere.

The following analysis of this mineral has been made by Dr. Fyfe:

Magnesia.....	69·75
Water.....	30·25
	<hr/>
	100·00

(Edinburgh Philosophical Journal.)

IV. Results of Experiments, with a Magnetimeter invented by Mr. Scoresby.

1. Iron bars become magnetical by position, excepting when placed in the plane of the magnetic equator; the upper end, as regards the position of the magnetic equator, becoming a south pole, and the lower extremity a north pole.

2. No attraction or repulsion appears between a magnetized needle and iron bars; the latter being free from permanent magnetism, whenever the iron is in the plane of the magnetic equator; consequently, by measuring the angle of no-attraction in a bar placed north and south, we discover the magnetic dip.

3. Before a magnet can attract iron, that is totally free from both permanent magnetism and that of position, it infuses into the iron a magnetism of contrary polarity to that of the attracting pole.

4. A bar of soft iron held in any position, except in the plane of the magnetic equator, may be rendered magnetical by a blow with a hammer, or other hard substance; in such cases, the magnetism of position seems to be fixed in it so as to give it a permanent polarity.

5. An iron bar, with permanent polarity, when placed any where in the plane of the magnetic equator, may be deprived of its magnetism by a blow.

6. Iron is rendered magnetical if scowered or filed, bent or twisted, when in the position of the magnetic axis, or near this position; the upper end becoming a south pole, and the lower end a north pole; but the magnetism is destroyed by the same means, if the bar be held in the plane of the magnetic equator.

7. Iron heated to redness, and quenched in water, in a vertical position, becomes magnetic; the upper end gaining south polarity, and the lower end north.

8. Hot iron receives more magnetism of position than the same when cold.

9. A bar-magnet, if hammered when in a vertical position, or in the position of the magnetic axis, has its power increased if the south pole be upward, and loses some of its magnetism if the north end be upward.

10. A bar of soft steel, without magnetic virtue, has its magnitude of position fixed in it by hammering it when in a vertical position; and

loses its magnetism by being struck when in the plane of the magnetic equator.

11. An electrical discharge, made to pass through a bar of iron, devoid of magnetism, when nearly in the position of the magnetic axis, renders the bar magnetic; the upper end becoming a south pole, and the lower end a north pole; but the discharge does not produce any polarity, if the iron be placed in the plane of the magnetic equator. The effects appear to be the same, whether the discharge be made on the lower or upper end of the bar, or whether it is passed longitudinally or transversely through the iron.

12. A bar of iron possessing some magnetism has its polarity diminished, destroyed, or inverted, if an electric discharge be passed through it, when it is nearly in the position of the magnetic axis, provided the south pole of the bar be downward, while its magnetism is weakened, or destroyed, if it receive the shock when in the plane of the magnetic equator.

13. Iron is rendered magnetical, if a stream of the electric fluid be passed through it, when it is in a position nearly corresponding with that of the magnetic axis; but no effect is produced, when the iron is in the plane of the magnetic equator.—(Edinburgh Philosophical Journal.)

V. Dissection of Crystals.

M. Faraday found, that those specimens of sulphuret of antimony which are crystallized in large crystals, crossing each other, admirably illustrate Mr. Daniell's mode of displaying crystalline texture by dissection. On introducing such a piece of sulphuret into a portion of fused sulphuret, and continuing the heat, it begins to melt down; but so far from this taking place uniformly at the surface, crystals will sometimes be left more than half an inch long projecting from it; and in other places, the cavities left by fused crystals will be so large, and have such perfect surfaces, that the angles they form with each other may be readily ascertained. In order to observe these effects, it is only necessary to remove the half fused piece of sulphuret from its hot bath, and allow it to cool.—(Institution Journal.)

ARTICLE XIV.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. Swainson is preparing for publication, *Exotic Conchology*, a Work to consist of coloured Plates of rare and nondescript Shells.

A General History of Birds, by J. Latham, MD. &c. Author of the Synopsis of Birds, &c.

The Fossils of the South Downs; or Outlines of the Geology of the South Eastern Division of Sussex, by Gideon Mantell, FLS.

JUST PUBLISHED.

A Manual of the Diseases of the Human Eye, from the best National and Foreign Works; translated from the German of Dr. Weller, and

illustrated with Cases. By G. C. Montearth, M.D. Illustrated by four highly coloured Plates, and one Plate of Instruments. 2 vols. 8vo. 14. 10s.

Practical Observations on Midwifery. By John Ramsbottom, M.D. 8vo. Part I. 10s. 6d.

The History of the Plague, as it lately appeared at Malta, Gozo, &c. By J. D. Tully, Esq. 8vo.

A Practical Treatise on the Inflammatory, Organic, and Sympathetic Diseases of the Heart; also on Malformation, Aneurisms, &c. By Henry Reeder, M.D. RMS. Edin. and MCS. Lond.

The ninth Number of Ferussac's Natural History of Land and Freshwater Shells is just published.

Useful Knowledge, or a Familiar Account of the various Productions of Nature, Mineral, Vegetable, and Animal. By the Rev. W. Bingley, AM. Third Edit. 3 vols. 12mo. 17. 1s.

Transactions of the Cambridge Philosophical Society. Vol. I. Part I. 4to. 17.

Illustrations of the Linnæan Genera of Insects. By W. Wood, FRS. FLS. Part I. with 14 coloured Plates, 5s. to be completed in six monthly Parts.

The Natural History System of Mineralogy. By Frederick Mohs, Professor of Mineralogy, Freiberg. 8vo. 6s. 6d.

ARTICLE XV.

NEW PATENTS.

Thomas Masterman, of Broad-street, Ratcliffe, common brewer, for certain machinery for the purpose of imparting motion to be worked by steam and water, without either cylinder or piston, and with less loss of power than occurs in working any of the steam-engines now in use.—Feb. 10, 1821.

Robert Stein, of Walcot-place, Lambeth, for certain improvements in steam-engines.—Feb. 20.

James Foster, of Stourbridge, iron-master, for certain improvements in the manufacture of wrought malleable iron.—Feb. 20.

Henry Penneck, of Penzance, MD. for an improvement, or improvements, of machinery to lessen the consumption of fuel in working steam-engines.—Feb. 27.

Robert Burton Cooper, of the Strand, London, for improvements on, or a substitute for, stoppers, covers, or lids, such as are used for bottles, tobacco, and snuff-boxes, ink-holders, and various other articles requiring stoppers, covers, or lids.—March 3.

Jonathan Dickson, Holland-street, Blackfriars, for valuable improvements in the means of transmitting heat, and also in the means of transmitting cold from one body to another, whether solids or fluids.—March 5.

William Frederick Collard, of Tottenham-court-road, for certain improvements on musical instruments called piano-fortes.—March 8.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1821.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at	
		Max.	Min.	Max.	Min.			9 a.m.	
3d Mon.									
March	1 S	E	29.83	29.44	48	32	—	03	94
	2 S	W	29.90	29.82	51	40	—	26	84
	3 S	W	29.82	29.70	52	45	—	—	100
	4 S	W	29.83	29.70	54	33	—	31	82
	5 N	E	30.02	29.83	35	30	—	—	80
	6 S	E	29.89	29.63	45	35	—	32	88
	7	W	29.65	29.35	58	44	—	22	86
	8 N	W	29.65	29.32	52	40	—	13	83
	9	W	29.67	29.63	54	47	—	08	76
	10 S	W	29.84	29.67	58	40	—	05	78
	11 S	W	29.97	29.84	55	33	—	—	67
	12	Var.	30.05	29.97	54	39	—	08	90
	13 S	W	30.11	30.05	56	36	56	03	89
	14	N	30.38	30.11	49	24	—	—	76
	15 N	E	30.38	30.32	51	26	—	—	78
	16	Var.	30.30	30.10	53	24	—	—	80
	17 N	E	30.10	29.57	55	37	—	12	79
	18 N	W	29.57	29.34	49	35	—	27	64
	19 N	W	29.36	29.31	45	34	—	—	61
	20 N	W	29.36	29.35	47	35	59	—	58
	21 N	W	29.71	29.36	46	34	—	—	61
	22 N	W	30.10	29.71	47	26	—	—	64
	23 N	W	30.10	29.88	47	35	—	—	64
	24	S	29.88	29.47	48	42	—	20	58
	25 S	W	29.68	29.47	51	32	—	09	88
	26 S	W	29.66	29.39	50	38	—	07	67
	27 S	W	29.39	29.26	48	33	—	—	62
	28 S	E	29.26	29.12	58	41	55	43	87
	29 S	W	29.65	29.12	47	34	—	07	36
	30 S	W	29.65	29.42	50	36	—	10	78
	31 S	W	29.60	29.39	51	32	20	11	86
			30.38	29.12	58	24	1.86	2.92	100—58

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Third Month.—1. Cloudy. 2. Showery afternoon. 3, 4. Rainy. 5. Cloudy. 6. Rainy. 7. Fine day: rainy evening. 8. Fine. 9. Showery. 10. Showery. 11, 12. Fine. 13. Fine morning: a slight shower of hail about noon: lunar halo in the evening. 14. Fine: lunar corona. 15. White frost: fine: a very distinct lunar halo, slightly coloured. 16, 17. Fine: white frosts in the morning. 18. Boisterous: frequent showers of mingled hail, snow, and rain. 19. Windy: hail. 20. Windy: cold. 21. Cloudy. 22. Hail. 23. Fine. 24, 25. Showery. 26. Fine day: boisterous night. 27. Boisterous. 28. Rainy: windy. 29. Showers. 30. Fine: rain at night. 31. Rainy.

RESULTS.

Winds: N, 4; NE, 3; SE, 8; S, 1; SW, 12; W, 2; NW, 7; Var. 2.

Barometer: Mean height

For the month..... 29.710 inches.

For the lunar period, ending the 26th..... 29.765

For 14 days, ending the 4th (moon south)..... 29.999

For 14 days, ending the 15th (moon north)..... 29.866

Thermometer: Mean height

For the month..... 49.758°

For the lunar period..... 41.448

For 30 days, the sun in Pisces..... 39.383

Evaporation..... 1.66 in.

Rain..... 3.02

Mean of hygrometer..... 77°

* * The mean temperature of this month, by Six's Thermometer, at *Tottenham*, was 43.612°; and the rain for that station, 3.44 inches.

Laboratory, Stratford, Fourth Month, 20, 1821.

R. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1821.

ARTICLE I.

A Mathematical Inquiry into the Causes, Laws, and principal Phenomena of Heat, Gases, Gravitation, &c. By John Herapath, Esq. (In a Letter to D. Gilbert, Esq. MP. VPRS. &c.)

(Concluded from p. 351.)

General Scholium.

Sudden condensation in all gaseous or aeriform bodies produces heat, and sudden rarefaction cold; but if the condensation or rarefaction be made slowly, no perceptible change in the temperature takes place. These are natural consequences of our theory of the constitution of gases. For if it be a condensation, by the motion inward, for instance, of one of the sides of the containing body, it is evident that the particles which strike against this side and are reflected back among the rest, will no longer be reflected by the same force only, with which they were previous to the commencement of the condensation, but by a force, which will augment their velocity individually by a quantity equal to the velocity of the moving side. And this excess of velocity being distributed to the rest of the particles, or communicated to them by the continuance of the stroke, will generate an excess of temperature throughout the medium proportional chiefly to the velocity of condensation. On the contrary, in the case of rarefaction, those particles, instead of returning among the rest with the celerity with which they did before the side began to move, will now return with a diminution of their velocity; equal to the velocity of the moving side. This diminution must, therefore, cause a decrease of tempera-

ture in the medium, in the same manner as the excess of velocity would cause an increase. In both these cases, the excess or defect of temperature will be proportional to the celerity of condensation or rarefaction, supposing the time occupied by the change of volume to be of sufficient length for the stroke to be propagated to all parts of the medium. Therefore, if the celerity of the stroke be great, a great change in the temperature will ensue; and if it be small, there will be little or none.

Our theory of gases likewise explains another phænomenon, which has usually been attributed to a different cause. It is well known that if two gases be placed in any position with respect to each other, and have ever so small a communication, they will intermix, and become equally diffused the one throughout the other. This has been accounted for on the principle that all gases have a slight affinity for one another, or in other words, that though the parts of all gaseous bodies individually considered mutually repel each other, yet the parts of different gases, however great or however small that difference may be, mutually attract each other. It is repugnant to my intention to attempt any refutation of this idea, or to make any observations on the confusion that would result from thus burthening nature with hypotheses; I shall, therefore, merely show that the phænomenon in question is an easy consequence of our theory. Suppose that portions of the surfaces of two media are exposed to each other, and that parallel to these portions the media are divided into strata. Then because a difference in the numeratons, a difference in the motions of the particles of the two strata, or a cornered irregular figure in the particles, would render it impossible for the particles of one stratum to be so reflected by the particles of the other, that each stratum would retain its particles entire and unmixed with those of the other, the two strata would intermix; and would not arrive to a state of equilibrium action, until the particles were uniformly and equally disposed in each stratum. For the same reasons, these newly compounded strata would mix with their next; and thus it would go on stratum intermixing with stratum, until an equilibrium action throughout was restored, by the equal and uniform intermixture of the two gases.

By the same principles it is easily perceived, why gases so readily acquire the temperature of the surrounding bodies; why they transmit temperature so rapidly and so feebly in right lines; and why, according to the manner in which Mr. Leslie, Sir H. Davy, and MM. Dulong and Petit, have made their experiments, it is owing to the gravitation of the gases alone, that the lighter gases cool bodies immersed in them quicker than the heavier. When I first turned my attention to the cooling power of gases, I was surprised to find from my theory, that all gases, under the same circumstances, ought to cool equally fast. This inference for some time perplexed me very much. As soon,

however, as I had read the ingenious treatise of Prof. Leslie on Heat and Moisture, and had considered anew the manner in which Sir H. Davy had conducted his experiments, which were about that time published, I saw that the theory and phænomena still agreed; and that the circumstances of my investigation precisely coincided with Mr. Leslie's cooling by gaseous pulsation; while the error that I had committed, with respect to Sir H. Davy's experiments, rested wholly on my not having taken into account the earth's attraction.

Their temperatures and elasticities being the same, the ratio of the numeratons of two homogeneous gases appears by our theory to be equal to the subduplicate ratio of the weights or specific gravities of equal volumes.* Supposing, therefore, that oxygen and hydrogen are homogeneous (the truth of which in oxygen I much doubt), the numeratom of the former will be quadruple that of the latter. So that if two in volume of hydrogen unite with one in volume of oxygen to form water, the atoms of oxygen will be double in number those of hydrogen; and the numeratom of the compound gas before being condensed will be the geometrical mean between the numeratons of the two simples. It has commonly been conceived that two atoms of hydrogen and one of oxygen form a particle of water; but whether this, or whether the result of our theory, or whether neither of them be true, it is out of our power to determine. Such kind of speculations transcends the corroboration of any experiment yet devised. I simply mention this theorem, which is one among several that I have investigated, relative to the mixture of gases, to give some small idea of the powers of our theory of the universe for unravelling the secret operations of nature.

By the same theory I have found that if equal portions of the same gas be mixed together at different temperatures F , F_1 , accounting in degrees of Fahrenheit, according to the indications of the air thermometer, the mean resulting temperature F_{11} of the mixture, measured on the same scale, and no extraneous force interfering, will be equal to $(448 + F) \times$

$$\left(\sqrt{\frac{448 + F_1}{448 + F} + 1} \right)^2 - 448, \dagger \text{ supposing } F \text{ to represent the}$$

degrees at the lower, and F_1 those at the higher temperature. And if the volumes, instead of being equal, be in the ratio of n to 1 when the temperatures and elasticities are equal, then by a mixture at different temperatures, $F_{11} = (448 + F) \times$

* This theorem was published in a more general form in the *Annals of Philosophy*, for July, 1816.

† 448 represents the degree of absolute cold below the zero of Fahrenheit, employing the air thermometer, and estimating in Fahrenheit's scale of degrees.

$\left(\sqrt{\frac{448 + F_1}{448 + F_2} \cdot \frac{n+1}{n}} \right)^2 - 448$. Hence if equal portions of the

same gas be brought to the temperatures of water freezing and boiling; and mixed together, $F = 32$, $F_1 = 212$, and F_2 will be found $= 118\frac{1}{2}^\circ$. Between these temperatures, the variations in volume of mercury and gas have been found to be perfectly equal; therefore, if, instead of gas, equal portions of mercury, water, or any other fluid be mixed together, the temperature of the mixture by Fahrenheit's mercurial thermometer should be $118\frac{1}{2}^\circ$. De Luc, as I have read in Dr. Ure's paper published in the Transactions of the Royal Society for 1818, by mixing equal portions of water at those temperatures, obtained a mean of 119° , which so nearly agrees with what has been computed from theory, that the difference may reasonably be ascribed to the observation. It must, however, be confessed, that the experiments of Dr. Crawford do not confirm those of M. De Luc. According to that philosopher, the resulting temperature is 122° ; that is, the arithmetical mean of 212° and 32° . It is, however, a question, whether the experiments of De Luc are not entitled to considerably more confidence than those of Crawford. The object of the two philosophers was the same; namely, to determine, whether equal increments of temperature are accompanied with equal expansions of volume; and hence I should think that De Luc, as soon as he perceived the anomaly, left no means unessayed, which repeated and careful experiments could afford him, to satisfy himself of its existence, before he ventured to publish a circumstance so contrary to the general opinion of philosophers. But with respect to Dr. Crawford (an account of whose experiments also on this subject I have unfortunately not seen), he appears, from M. Biot's *Traité de Physique*, tom. iv. p. 700, not to have followed one of the most likely methods to ensure accuracy: "Il a cherché à le faire (that is, a correction for the loss of temperature) en observant la loi du refroidissement progressive du mélange, et s'en servant pour restituer, par le calcul, le chaleur qu'il avait dû perdre dans les premiers momens." It would, in my opinion, have been much better to prevent, than to have had to account for, this loss. But without discussing the merits of the methods of either philosopher, the subject of determining whether the increments of temperature and volume are proportional, as it respects future discoveries, is of the first consequence; and I, therefore, beg to submit the settling of so important a point to the consideration of the Royal Society; and shall feel myself honoured if it appear worthy of their attention. I should not trouble the Royal Society if I had apparatus upon which I could depend for trying an experiment of such delicacy. Unhappily I have not only no thermometers myself sufficiently good, but I know of no one in

this neighbourhood who has; nor is there at present any Society in Bristol to which I could apply for assistance, though from the spirited exertions now making, there is reason to believe, we shall soon possess one that will be every thing we could wish.

For making an experiment* of this kind, I know of no body better than mercury, which is fluid at a very low, and boils at a very high, temperature. If two large and equal portions of this fluid, at the temperatures intended, were rapidly poured into a thin smooth vessel, previously brought to the expected mean temperature of the mixture, or a degree or two higher; and if the temperature of the mixture was taken by two thermometers, the one two or three degrees lower, and the other about as many degrees higher than the computed mean, I think the obstacles to success, pointed out by M. Biot, might be avoided; and this great problem, with one or two repetitions of the experiment, be resolved to a considerable degree of accuracy. Could the experiment be made in an atmosphere just above the temperature of the mixture, it would prevent any error arising from radiation, &c.; and if the vessel into which the portions of mercury are poured be but little larger than sufficient to hold them, and spherical, it would subject the experiment to the least influence of the matter of the vessel. But if the experiment cannot be made in such an atmosphere, it would be advisable to cover the whole apparatus for a few minutes, until the temperature could be equally diffused, with a polished metal cover, which would tend to prevent any loss of temperature by the contact of air, or by radiation. These and some other precautions, which circumstances would easily suggest, would, it appears to me, ensure to the experiment all the success of which, perhaps, it is capable.

When the difference between the temperatures of the portions of fluid mixed exceeds 180° of Fahrenheit, the arithmetical and computed means differ from each other by a quantity which increases much faster than the difference of the temperatures. If equal portions be mixed at the temperatures of -33° and $+300^{\circ}$, the value of F_{m} will come out 121.32° , while the arithmetical mean is 133.5° ; that is, upwards of 12° in excess. Within these limits, mercury and air go on nearly *pari passu*, the volume of mercury at the higher temperature being only about 2.34° greater than that of air. But our theory seems to be independent of all considerations of proportional variations of volume; the only precaution necessary appears to be to measure the temperatures by the air thermometer instead of the common mercurial. If mercury at -7° and $+641^{\circ}$ be mixed, the computed mean temperature will be no less than 36° below the

* I have since succeeded in some experiments of this kind in high temperatures with mercury, and have found the mean error of my theory by seven experiments of my own, and two of De Luc's, not more than about the one-tenth of a degree; the mean error of the old theory being eight degrees.

arithmetical mean. Such a difference as this would be extremely striking and decisive; but whether those extreme temperatures can be operated on, I leave others to determine. Other circumstances being alike, the most advantageous method of making the experiment is when the volumes mixed together are equal.

In any homogeneous fluids the same principles will enable us to discover the ratios of the numeratons and the magnitudes of the particles. On the supposition that mercury and water are homogeneous fluids, I have found, from the best experiments I can procure, that the ratio of the numeratons of mercury and water is about equal to that of 1 to 2, and the ratio of the magnitudes of the particles equal to about that of 27 to 1, and, therefore, the ratio of their diameters, supposing them similar, about that of 3 to 1. This greater numeraton of the water is indicated by the mean temperature of the mixture of equal parts of mercury and water always being in favour of the temperature of the water; and the excess of magnitude in the particles of mercury, by its less disposition to be affected in volume by changes of temperature.

Taking these numbers for correct, I find that if a given volume of mercury at the temperature of 100° Fahrenheit be mixed with an equal volume of water at the temperature of 40° , the temperature of the mixture should be $59\frac{1}{4}^{\circ}$; by Dr. Henry, it is 60° . And if the same temperatures be taken, but the water be put at the higher, and the mercury at the lower temperature, the mixture should be at $79\frac{1}{4}^{\circ}$: Dr. Henry says it is nearly 80° . If two volumes of mercury and one of water be mixed at the temperatures of 100° and 40° , it matters not whichever of them has the higher temperature, the temperature of the mixture ought to be about $69\frac{1}{4}^{\circ}$: by the above author, it is 70° . An equal coincidence holds good in the other cases mentioned by Dr. Henry. Therefore, what has been usually attributed to "the capacity of bodies for caloric," appears to be explicable on the theory of their numeratons, that body whose particles are the smaller and more numerous, being that which has been supposed to have the greater "capacity for caloric;" and *vice versa*. But to be more certain of this, it would be necessary to make fresh experiments with a greater difference of temperature. Supposing the aforesaid ratio of the numeratons to be right, and that water and mercury are homogeneous fluids, I find, if equal parts of mercury and water are brought to the temperatures of 40° and 212° , and then mixed together, the resulting temperature should be $151\frac{1}{4}^{\circ}$, when the water has the higher temperature, and $94\frac{1}{4}^{\circ}$ when it has the lower. The arithmetical mean of 212° and 40° is 126° , which is $31\frac{1}{4}^{\circ}$ above $94\frac{1}{4}^{\circ}$, and only $25\frac{1}{4}^{\circ}$ below $151\frac{1}{4}^{\circ}$. If the temperatures be 40° and 200° , the mixture will be 91° and 144° , in which the distances from the mean are -29 and $+24$. I have not tried these experiments for the reasons I have given before; but I would thank any one who would; and should be still more obliged to him if he would have

the goodness to send me the results of these and any accurate experiments of the kind.

The same principles might easily be extended to solids; but sufficient has been said for the limits of a mere memoir on this part of the subject. I shall, therefore, now briefly, offer one or two observations for which a distinct hypothesis has been contrived; namely, the hypothesis of "latent heat." It is generally admitted, I believe, that when gases or vapours become fluids, two or more of their atoms or particles unite together to form a particle of the fluid; and that when fluids become solids, a still further aggregation of the particles takes place; so that fluids are solids resolved into simpler elements, and gases solids resolved into still simpler, or, perhaps, their simplest elements. This being granted, it follows, that if two or more atoms or parts of the gas unite together into one, the motion of this one, being compounded of the motions of the others which compose it, will be greater than the motions of either of the constituent parts before the aggregation. Therefore, if the temperature of the bodies in each of the three states be measured by the motions of their constituent parts, then when a gas by any means becomes a fluid, the aggregated parts of the fluid will have a greater motion than the parts of the gas; and consequently its temperature for a little while will be higher. For the same reason the temperature will be raised in passing from a fluid to a solid state; and in all cases the increase of temperature in passing from a gaseous to a fluid, or from a fluid to a solid state, will be proportional to the aggregation of the particles; so that in either change of state, the greater the number of particles that unite, the greater the rise of temperature. And conversely, an augmentation of temperature being produced by a union of particles, a diminution must be produced by a separation of them; and this diminution will be the greater, the greater the number of parts into which a particle is divided. Therefore the constitution of things being such as I have supposed, a diminution of temperature will accompany a change of state from a solid to a fluid, or from a fluid to a gas or vapour. But though with these views an increase of temperature generally accompanies a change of state, when by that change the solidity is increased, or *vice versa*, yet this is not of necessity a law of nature. Circumstances might be easily varied, so that an augmentation of solidity by a change of state may be attended with a diminution of temperature, or the contrary; and thus the phænomena of the increase of temperature at the explosion of gun and all fulminating powders, as well as other anomalies, might be accounted for. These, however, are things which the importance of others will not allow me now to stop to explain. The same reason prevents me from entering into an explanation of the cause of that apparent loss of temperature in the experiment respecting the admixture of equal parts in weight of water at 172° , and snow or ice at the temperature of water freezing, and other similar phæno-

mona. A full development of these things, and the inferences which might be drawn from them, would swell the memoir considerably beyond the limits of an ordinary communication.

I come now to the law of gravitation or attraction.* If a particle of matter were put in the centre of such a fluid medium, as we have supposed in the fifth postulatam, and if by its agitations propagated every way about, and duly counterbalanced at the limits of space, it kept the minute parts of this fluid medium in continual motion, then would a spherical atom be urged towards

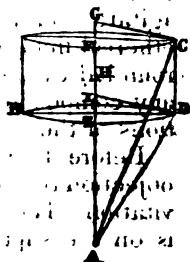
the central particle by a force which is as $\frac{m}{x^2}$, in which a represents the intensity of the central agitation, m the magnitude of the impelled atom, and x the distance of the centres of the two bodies.

When the attracted atom is not a sphere,† its law of gravitation becomes more complex; and in a general way is not to be obtained in algebraic terms; for it is dependant not only on the intensity of the central agitation, the magnitude of the attracted atom, and its distance from the central one, but likewise on its figure and position. In all cases, however, when the attracted atom is exceedingly small and at any sensible distance, or when it is at a distance sufficiently great to render the influence of its magnitude insensible, its gravitating force will be as its magnitude and the intensity of the central agitation directly, and the square of its distance inversely. Consequently, if instead of a single central particle, there were a great number distributed so as to form a uniform sphere, and in such a manner that the agitations of each may take effect, the force of the impulsion on the solitary atom, Principia, prop. 74, lib. 1, would be as $\frac{m A}{x^2}$; in which m is the mass of the attracted atom, A the agitat-

* That no one may have to charge me with a desire of making innovations, I have retained the terms attraction, centripetal force, &c. though from the cause of the phenomena, as laid down in this memoir, probably impulsion, adulsive force, &c. or, some other words having a nearer affinity to the primitive cause, would be preferable.

† The simplest case after a sphere is, perhaps, that of a cylinder, having the centre of agitation in its axis produced. Let BC be a cylinder, EF its axis, and A the centre of agitation. Draw AC , AD , and in AF take AG , AH , respectively equal to AC , AD ; join CG , and complete the parallelogram IC . Then will the gravitating force of the whole cylinder towards A be as the rectangle of the agitation at A , and the distance HL . But if the semidiameter ED be vastly small, compared to AE , the gravitation is as the agitation at A , and the magnitude of the cylinder directly, and the rectangle AE , AF , inversely. Therefore, if EF be likewise very small in comparison of AE , the gravitation is directly as the agitation at A , and the magnitude of the cylinder, and inversely as the square of its distance from A .

When the impelled body is a parallelepipedon, its gravitation is as the logarithm of a function of its dimensions and distance from the central particle, raised to a power equal to the intensity of agitation. And when it is so small, or at such a distance from the centre, that its dimensions are insensible in respect of that distance, its gravitation is proportional to its magnitude and the intensity of the agitation directly, and the square of the distance inversely.



ing force of the whole central body, and x the distance between the centres of the atom and compound body. And since this is the law of gravitation of a single atom, it follows, by prop. 75, lib. 1, Principia, if a number of them were equally distributed throughout any spherical space, so that they could be individually acted on by the fluid medium, the impelling force on the whole body would be as $\frac{A^2 b}{x^2}$; in which x is the central distance of the two bodies, and b the quantity of matter in the attracted body. For the same reason if B be the mass of the central body, and A the whole agitating force of the other, its attraction on the central body will be as $\frac{A^2 B}{x^2}$. But the whole agitating force of a homogeneous spherical body, other things being the same, is as its temperature T , its numerator N , and its volume V conjointly; therefore, T being the temperature of the body B , its absolute attractive force on b will be as $\frac{T N V b}{x^2}$, and its accelerative force as $\frac{T N V}{x^2}$. And t, n, v , representing the like things of the other body b , its absolute attractive force on B will be as $\frac{t n v B}{x^2}$, and its accelerative as $\frac{t n v}{x^2}$.

Now Newton has shown, p. 242, of his Optics, bodies to be so rare that water has at least 40 times more pores than solid parts; and Biot, in his *Traité de Physique*, tom. iv. pp. 124, 125, has carried the idea so far, as even to suppose the solar system itself to be but one great particle relatively to other larger and rarer systems. Without inquiring into the merits and probability of this bold idea, there is every reason to believe with him, that "il se pourrait même que dans les corps qui nous paraissent les plus denses, la capacité des interstices surpassât plusieurs milliers des fois le volume des particules matérielles;" or, at least, that the densest bodies we know of do indeed contain vastly more pores than solid parts. Therefore, it follows, that every one of the particles of any compound body might agitate and produce those affections I have imagined on the ethereal fluid, very nearly the same as if it was alone and unobscured by the other particles; and conversely, the particles of any compound body might be individually affected by the agitations of the medium, almost the same as if they were alone.

Before I proceed further, it is necessary to anticipate an objection that at the outset might be made to this theory of gravitation. The force of gravitation that has been here determined is on the supposition that the impelled body is in a state of quiescence; and it might, therefore, be conceived, that the attraction would be less on a body moving towards the central body, and greater on one moving from it, which is contrary to

expression for the law of gravitation towards any spherical body, the gravitating forces, or the weights of any two spherical bodies towards any other body at equal distances, are as the quantities of matter in the attracted bodies. This law has been proved experimentally by Sir Isaac Newton; but though this be true, the converse case does not, according to our principles, hold good; namely, that the attractive forces of bodies are directly proportional to their quantities of matter. Our principles do not, therefore, corroborate Newton's third law of motion, respecting the equality of action and reaction in attracting forces; for by our theory a body might, by the agency of the fluid medium, be impelled towards another without any reciprocal action, which is by no means surprising, if we consider attraction not to be an inherent or essential property of matter, but merely the action of a third body. When, however, we take homogeneous* bodies of the same temperature, the equality of action and reaction seems to hold true; but in different bodies, differently composed, or the same body at different temperatures, a difference should be observed in their attractive forces proportional to the greatness of the dissimilarity. Other things being alike, the attractive forces should be proportional to the temperatures; so that the attraction of all bodies at the extreme temperatures of the fluidity of water, should have a ratio of about 6 to 7. Unfortunately the difficulties attending the corroborating by experiment of this part of our formula, much exceed those of the other; nor have I as yet devised any method of satisfying myself respecting it in the manner that I could wish.† But though great difficulties lie in the way of a numerical proof, there are some phænomena which seem pretty decisively to sanction the general result of our formula; namely, that an increase of temperature produces an augmentation of attraction. Thus Euler has found that the focus of a convex lens is contracted by a greater and extended by a less temperature; from which it plainly appears, that the refracting force is increased in the one and diminished in the other case. M. Laplace in the *Mécanique Céleste* has calculated the annual equation of the moon at $11' 22''$, while, by the best tables, it is only about $11' 8\frac{1}{2}''$. The difference between these quantities appears too much for the error of either observation or calculation, and therefore indicates an increased attraction in the perihelion, and a decreased in the aphelion earth. By all the calculations that have hitherto been made, a regular diminution in the attractive forces of the planets, reckoning from the sun, has been observed, which shows that the colder planets have less attraction than the

* By homogeneous solid or fluid bodies is here meant those all of whose parts are similar, both with respect to their constituent elements and the association of them.

† Since writing this, I have contrived a method, by a modification of Mr. Cavendish's apparatus, but have not yet met with a proper place and opportunity for putting it in practice.

warmer. Uranus alone, if future computations should verify the present, exhibits an exception to this rule. This is not indeed impossible, nor repugnant to our formula; it only requires a different constitution from the rest of the planets, such, for instance, as minuter particles; for then by reason of the coldness, they may be more numerous. Another argument in favour of our formula is the very small disposition to disturb that is found in comets.* This might in part arise from a difference of constitution, but it hardly seems reasonable to make it the only cause with all of them.

Granting that attraction and temperature are proportional, it follows that the polar parts of the earth being colder than the equatorial, the attraction of the whole earth, in places about the former, must be less than its attraction in places about the latter; and, therefore, the isochronal pendulum must be shorter under the poles than at the equator. On this account, therefore, if the earth was a sphere, its figure, as indicated by the pendulum, should be a prolate spheroid; and if the figure be an oblate spheroid, the measures by the pendulum would give either a prolate spheroid, a sphere, or an oblate spheroid of less eccentricity than it really is. Now, according to the best observations, the compression of the earth by the pendulum is about $\frac{1}{100}$, whereas by Newton, from the theory of gravity, it is $\frac{1}{230}$, and by most of the admeasurements it lies between the two, or about $\frac{1}{100}$ by Col. Lambton. But the same theory which indicates too small a compression with the pendulum, indicates also too large a one on the supposition of uniform gravity; for as the gravitation on the Newtonian theory is greater at the poles and less at the equator than it ought to be, Newton must, in his computation, have made the polar fluid canal too short, and the equatorial too long, and consequently the ellipticity of the earth too great. Hence our theory also indicates the cause of those discrepancies in the three methods of determining the figure of the earth, which have so much baffled mathematicians. The intervention of other things, and a want of extensive and correct observations on the mean temperatures in different latitudes, prevent me at present from attempting a numerical proof of these ideas. Humboldt's collection of experiments on the temperatures of different places, lately published, and the experiments made by Dr. John Davy in his voyage to Ceylon, with some others that have lately appeared, have indeed enabled me to fix very nearly † the law of

* "Il a plusieurs raisons (says M. Laplace) de croire que les masses des comètes sont très-petites, et qu'ainsi leur action est insensible."

† The formula for accurately expressing the mean annual temperature of different latitudes would be one of very great complexity, and, perhaps, in the present state of science, would surpass the powers of analysis to exhibit in a finite equation. Assuming the temperatures of the polar and equatorial regions at 0° and $81\frac{1}{2}^{\circ}$ of Fahrenheit, my formula will seldom differ more than a degree or two of Fahrenheit's scale from that lately given by Dr. Brewster, namely, temperature = $81\frac{1}{2}^{\circ} \times \cos. \text{latitude}$.

the variation of the temperature; but they want that extent and variety which are requisite to establish correctly the true mean temperature of the earth in different parallels. To settle this point well, a series of observations* are wanting, in the intersections of about every fifth or tenth degree of latitude and longitude on both sides of the equator, and as near to the poles as possible. Local circumstances will no doubt always interfere to prevent the perfect coincidence of any general law of temperature with observation, which is the case with the one that I have assigned. Indeed were it not so, and were the earth of a uniform constitution, it would be no difficult matter to determine its ellipticity from the temperature alone of different latitudes.

Philosophers inform us that the southern parts of the earth are colder than the northern. This might possibly be owing to the different structure of the superficial earth in the two hemispheres, with respect to land and water; but whatever be the cause, an inequality in the mean temperatures of corresponding latitudes cannot fail, if it be considerable, to affect the attraction of the earth at those places, and, therefore, the length of the pendulum, and consequently the figure of the earth, as determined by the pendulum. Whether this be really the cause of the irregularity of figure, which has been said to have been observed in the two hemispheres, I must leave until I have obtained data sufficiently correct to enable me to undertake a complete solution of the problem, respecting the attraction and figure of the earth. Upon the whole, however, this theory of gravitation seems to render it probable, not only that the earth differs in figure much less from a solid of revolution than has lately been imagined, but that its internal structure is also more uniformly homogeneous, than some philosophers have supposed.

Mathematicians will readily perceive, that a less temperature in the southern than in the northern hemisphere, if considerable, ought to affect the lunar theory, and accelerate the motion of the moon in Π and Θ , and retard it in \uparrow and Υ ; and that the equation thus produced ought to be the greatest when Θ falls in the vernal equinox, and the least when in the autumnal; and that other things being the same, it ought likewise to be greater about the time of the aphelion earth, and less about the time of the perihelion earth. The distance, however, of the moon from the earth, the small obliquity of its orbit to the equator, and the

* It might be advisable in future experiments to ascertain the temperature of the sea, and its diurnal variation, at different depths; for I do not remember to have seen any notice taken of these things in the experiments that I have hitherto read. One might indeed infer from Dr. Davy's experiments, that if there be a variation at any considerable depth below the surface, it is probably but trifling. It would, however, at least be satisfactory to decide this question by actual experiment; and besides, a knowledge of the temperature of the ocean at different depths, and in different places, would possibly enable philosophers to determine the internal temperature of the earth, and its law of variation towards the centre, as well as, perhaps, some other points of great interest to science.

trifling difference in the ratio of the temperatures, which a considerable thermometric difference in the two hemispheres would produce, conspire to show that the coefficient of such an equation must be exceedingly small, if not wholly insensible. I have, therefore, merely alluded to the subject, that philosophers might take an opportunity of considering it further, and of examining whether in the future improvement of the lunar tables, the unequal temperature of the different hemispheres, and the alternate increase and decrease of temperature in the same place, arising from the motion of the sun, are deserving of attention.

Returning to our law of gravitation, it is found that when the diameter of the attracted particle bears any sensible proportion to the central distance of the two particles, its gravitation, except in the case of its being a sphere,* no longer follows the law of the inverse squares of the distances, but a law, which increases the force of attraction much more rapidly as the particles approach. It is, therefore, evident that our theory of attraction is not only capable of expounding the gravitation of bodies at a great distance from one another, but also all that variety and increase of force which are observed in the particles of bodies when brought nearly into contact. For instance, if two particles approach each other with flattened surfaces, the intensity of their attraction when these surfaces are very near to each other, and the force of their cohesion when the particles are in contact, will be much greater than if the same particles could have presented spherical or more pointed surfaces, so as to touch but in one or two points. For the same agitations of the same particle will rarify the ethereal medium considerably more on the flattened side of the particle than on the pointed side (generally indeed more than in proportion to the greatest transverse sections of the particles); and, therefore, when the particles approach with their flattened sides, the medium between them is much more rarified, and its elasticity, therefore, much more diminished than they would be under similar circumstances of agitation between less superficial areas; and consequently the attraction of the particles, which consists in the excess of elasticity on the exterior and interior surfaces, is considerably augmented. And when the particles come into absolute contact, the flat sides, by displacing a greater portion of the medium from between them, will occasion the particles to be pressed together by a force, which, instead of being equal to about the

* Were the particles of fluid bodies nearly spherical, and were it by forces reciprocally proportional to the squares of their central distances that they are kept together, we should have $(S_1^{\frac{1}{3}} - S^{\frac{1}{3}}) Q = t_1, S_1^{\frac{2}{3}} - t S^{\frac{2}{3}}$; in which t_1, t , are any two temperatures, t_1 being the higher, S_1, S , the corresponding volumes of the fluid body, and Q an arbitrary quantity to be determined from the nature of the fluid. But the expansions determined from this formula are manifestly too little; since, if the particles attract one another with such forces, the fluid can never become gaseous. The true law of expansion in fluid and solid bodies, I can easily perceive, is connected with a gaseous problem which I have not yet resolved.

sum of the two excesses of the pressures on the further and nearer sides of the two particles, will be, perhaps, nearly equal to the sum of the two whole pressures on their further sides.

Different degrees of flatness will evidently cause different degrees of attraction and cohesion. But it is not to flatness alone that the cohesion, affinity, &c. of the particles may be owing; a proper adaptation of the parts or figure of one particle to the parts or figure of another, may as much conduce to the cohesion of the particles as flatness; and hence the greatest scope is afforded for expounding the various phænomena of chemical action. Suppose, by way of example, any compound particle whose component particles have a certain adaptation in figure, be brought into contact with any other particle, whose component particles have likewise a certain adaptation of figure; then if the figures of the elementary particles of either of the compound particles have, individually, or in any degree of aggregation, a much less adaptation to one another than they have to the figures of the elementary particles of the other compound particle, a new combination will instantly ensue; and one or more particles with properties, probably very different from those of the primitive compound particles, be formed. In this way, therefore, decompositions and precipitations may take place, or new compounds be made; and thus the various phænomena of chemical affinity may result entirely from the sizes and figures of the component particles. Besides, as an aggregation or division of the elementary particles will generally accompany every chemical change, so also will an elevation or a depression of temperature, and, therefore, the change that ensues in the temperature might serve for an index to point out, in some measure, the kind of change that has been effected; and by this means possibly some conjectures might be formed of the physical composition of many bodies.

I might now extend my observations to the solution of many other phænomena of nature. I might show that the fluidity of bodies arises from their particles not having a sufficient adaptation of figure, to make their cohesive tendency overcome the effects of their mutual impulses, due to the limiting temperatures at which they are fluid. I might show that the boiling points of fluids depend chiefly on the magnitude of their particles, and their freezing or congealing on the magnitude and adaptation conjointly. I might show how different combinations of the same elements might produce bodies differing essentially in colour, solidity, specific gravity, &c. such, for instance, as charcoal and the diamond; and, therefore, that notwithstanding such bodies may be composed of precisely the same constituent parts, a transmutation from the one to the other can, perhaps, never be expected, unless by decomposing them into their constituent elements, and then discovering some method of reasso-

acting those elements in the particular manner that nature has adopted. I might likewise unfold the cause of the opacity and transparency of bodies, and show that the phenomena of reflection and refraction consist in two distinct forces residing the one without the other, and acting upon the luminous particles separately and distinctly near the surfaces of bodies; that the figures of the molecules of light are the cause of the fits of easy reflection and transmission; of the phenomena of polarization; and combined with the figure of the integrant particles of doubly refracting crystals, likewise the cause of double refraction and its consequent phenomena. And by the same principles I might show how to explain the disproportional dispersion of light, and the various phenomena of diffraction, as well as the intersectionary bands lately discovered by Mr. Knox, and some circular and hyperbolic bands discovered by myself.* I might also, from our solution of the problem of gravitation, demonstrate, that the particles of light, however different in size, must be emitted with nearly equal velocities; that the velocity of the solar and astral light, conformably to phenomena, should be nearly equal; and that it is to the reciprocal action of the sun and stars, that gravitation itself, and consequently all the other phenomena, are owing; but as I have considerably extended this memoir beyond the limits I at first intended, and as my ideas on some of these subjects are not yet sufficiently matured, I have deemed it better to reserve what I may have further to say to a future period, when I may be enabled to put these and some other things together in a more methodical form.

For like reasons, I shall not now stop to inquire, whether the phenomena of magnetism, electricity, and galvanism, are referable to the same principles or not. Nor shall I attempt to make any observations on the still more metaphysical properties ascribed by Newton to our gravific medium; namely, whether "*Sensatio omnis excitatur; et membra animalium ad voluntatem moventur, vibrationibus hujus spiritus, per solida nervorum capillamenta, ab externis sensuum organis ad cerebrum, et à cerebro in musculos, propagatis;*" for these are speculations much too metaphysical for this place, and are besides things "*quæ paucis exponi non possunt.*"

I am, dear Sir, your obedient humble servant,

Bristol, Knowle Hill,† May 23, 1820.

JOHN HERAPATH.

* It was intended to make the substance of this communication a supplement to one containing an account of these bands, with a mathematical explication of their cause; but finding that I could not explain myself in so few words as I expected, I have thought it preferable to omit the optical part altogether.

† This was written at Knowle Hill, but the author now resides at Cranford, Hounslow, London.

Communication from the Rev. H. S. Trimmer respecting Mr. Herapath's Experiments.

SHALGROVE, Vicarage, Hinton, Middlesex, May 31, 1821.

When Mr. Herapath had determined to publish his valuable work his "*Mathematical Inquiry into the Causes, Laws, and principal Phenomena of Heat, Gases, Gravitation, &c.*" it appeared to him desirable that another paper containing the result of experiments made by him, and illustrating the accuracy of his views, should immediately follow; and he had accordingly made an arrangement for the appearance of this latter paper in the number for July.

The circumstances which have rendered an alteration in this plan necessary having originated with me, I consider it proper to apologize to you for any inconvenience which may have arisen to yourself, or for the disappointment which may be felt by your readers from my withholding this paper, and placing it in a different channel.

It has now been presented to the Royal Society by Davies Gilbert, Esq. MP. VPRS. &c. who received it from my hands.

I am, Sir, your obedient humble servant;

H. S. TRIMMER.

ARTICLE II.

Analysis of Verdigris. By R. Phillips, FRSE. &c.

HAVING lately had occasion to make some inquiry into the different methods of preparing acetic acid, my attention was of course particularly directed to acetate of copper, as one of the substances from which it has been often procured. Upon referring to the more recent chemical authors to ascertain its composition, I could not find that any analysis of it had been made since that given by Proust, according to which crystallized acetate of copper consists of

Acetic acid and water.....	61
Peroxide of copper.	39

100

From this statement, it is not possible to learn the quantity of real acid contained in the salt; I made, therefore; some experiments to determine this point. Acetic acid does not form with any substance a sufficiently insoluble compound to enable us to determine its equivalent with precision, nor can the whole of it be obtained by distillation; I adopted as a substitute the following method: One hundred grains of crystallized acetate of copper were dissolved in distilled water; excess of hydrate of lime was added to the solution, and the mixture boiled. The oxide of

copper precipitated, and the undissolved lime, were separated by the filter; through the clear solution, containing excess of lime, I passed a current of carbonic acid until the lime was precipitated, and I then heated the solution to separate any carbonate of lime which might have been redissolved by the carbonic acid. Having filtered the solution, I found it to be neutral acetate of lime, and then decomposed it with carbonate of soda, which gave 48·5 grs. of carbonate of lime. On repeating the experiment, I obtained 48·6 grs. the mean being 48·55. One hundred grains of crystallized acetate of copper were dissolved in water, and boiled with excess of potash. The peroxide of copper precipitated, weighed, after washing and drying, 38·9 grs. On repetition I procured 39·5 grs. giving a mean of 39·2 grs.

According to Dr. Thomson, an atom of acetic acid weighs 63·75; and I am satisfied, from various experiments, that it is very nearly correct; and if we consider this acid to be composed of three atoms of hydrogen, three of oxygen, and four of carbon, as is generally admitted, it will be represented by 63·96 on Dr. Wollaston's scale, agreeing almost precisely with Dr. Thomson's determination. If then 63 of carbonate of lime, the number representing it on the scale, give 63·96 for acetic acid, 48·55 the carbonate of lime resulting, as above described, from the treatment of 100 grs. of acetate of copper, will indicate 49·2 of acetic acid; so that we may consider crystallized acetate of copper as composed of

Acetic acid	49·2
Peroxide of copper	39·2
Leaving for water	11·6
	<hr/> 100·0

If, as is generally admitted, peroxide of copper be a compound of two atoms of oxygen 20, and one atom of copper 80, the atomic constitution of verdigris will be :

	By theory.	By experiment.
2 atoms of acetic acid	127·92	128·84
1 atom of peroxide of copper. ..	100·00	102·65
3 atoms of water	33·96	30·39
	<hr/> 261·88	<hr/> 261·88

Dr. Thomson has lately shown, that the salt called blue vitriol is a bisulphate of copper; and in addition to the reasons which he has assigned for this opinion, I may add that if finely divided carbonate of lime be added to a solution of it, insoluble sulphate of copper is precipitated with effervescence, and as the same effects are produced with the soluble nitrate and muriate of copper, and with the acetate also, although very slowly, I think we may conclude, that the soluble nitrate and muriate, as well as the sulphate and acetate, are bisalts.

ARTICLE III.

*Observations upon the Gas Blowpipe, and upon some of the more remarkable Results which have been obtained in using this Instrument during a Course of Five Years, in which it has been constantly employed; being a Continuation of former Remarks upon the same Subject.** By Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge; Member of the Royal Academy of Sciences at Berlin, &c. &c.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Cambridge, May 12, 1821.

A PERIOD of five years has now elapsed since I first began a course of experiments with the *gas blowpipe*. In all this time those experiments have been of a public nature. Upwards of 100 persons were present, *March 15* of the present year, when the metallic base of *barytes* was revived and exhibited. It had been cut by a file in three places; and it presented such a high degree of metallic lustre that it seemed as if the points of three iron nails had been similarly acted upon by the file. This metallic body being then left covered only by a glass vessel, speedily became oxidized, and fell into a white pulverulent earth of *barytes*. In this experiment, no oil had been used to mix with the *barytic earth* for the purpose of making its particles adhere; it was taken from a glass vessel in a compact state; as it had been prepared with the utmost care by Messrs. *Allen*, chemists, of London; and was exposed, *per se*, to the flame of the *gas blowpipe*, supported in a pair of forceps, the points of which were made of *slate*. The reasons which have given rise to the opinion that the *metallic substance* thus so often exhibited, is, in fact, the *metal of barytes*, are simply its property of rapidly decomposing water and atmospheric air; added to the perfect *metallic lustre* which it discloses to the action of a file, or other sharp instrument. In addition to which may also be mentioned the appearance which it exhibits prior to its being cut. It has then a highly *metallic surface* resembling the stalactites of the black oxide of *manganese*; but of a jet-black shining colour, with occasional *metalline particles*, disposed in a dendritic crystallization upon the dark surface. If, hereafter, chemists should determine that these characters are not of themselves sufficiently decisive as to its metallic nature, still the name of *plutonium*, which, in a former communication, I ventured to give to this appearance, may be considered as useful for its distinction, because, whatever be its real nature, it is a

* See *Annals of Philosophy*, x. 373, xiv. 143, &c.

result of fusion possessing properties of a peculiar nature. It differs from *wood-tin*, which exhibits, after fusion, the same degree of *metallic* lustre, but is still in the state of an *oxide*. Being guided by no other views than those which may conduce to the interests of science and the advancement of truth, after once more calling the attention of your readers to a subject not unworthy of their regard, it is my intention to consign to them its future and further consideration; briefly adding what, in the course of the last five years of experience, I have discovered to be the cause of failure in the attempts made for obtaining this effect of fusion in *barytes*; and moreover a few remarks upon some other curious phenomena which the use of the *gas blowpipe* has developed.

In the first accounts which I published of experiments with this blowpipe, the propriety of mixing the *oxygen* and *hydrogen* gases in the exact proportion for forming water was always insisted upon; because, when the *hydrogen* gas is only added in slight excess, the mixture either will not burn at all, or the temperature of the flame is greatly diminished; upon the elevation of which temperature the success of many experiments depends. There are some experiments for which only *hydrogen* gas alone may be employed combined with a small portion of atmospheric air; or *hydrogen* gas uncombined with any other aeriform fluid; but the temperature is then greatly lowered; and hence may be perceived the impropriety of naming this instrument the *oxy-hydrogen* blowpipe; because it is adapted to gases of all kinds; whether in a state of explosive mixture or not; some being used, as, for example, *oxygen* gas, not for combustion, but merely as a supporter of combustion, to propel the flame of a combustible body, as *alcohol*. It is on this account that I have preferred calling it the *gas blowpipe*; being an appellation of a more general nature; applicable, not only to the use which I have made of it in burning the gaseous constituents of *water*, but to all its operations with condensed gases of whatsoever nature they may be. But to return: I have since found that for communicating the utmost elevation of temperature to the flame of mixed *oxygen* and *hydrogen* gases, it is better to add the *hydrogen* gas in greater excess than was at first thought to be necessary: instead of *two* parts by bulk of *hydrogen* to *one* of *oxygen*, the proportion of *nine* to *four* is greatly to be preferred; as, for example, *nine* pints, or measures, of *hydrogen* gas, to *four* pints, or measures, of *oxygen* gas.* The operator upon opening the valve, to set fire to this mixture, will immediately perceive how much more explosive it is than any other; by the rapid snapping noise of the flame, caused by successive detonations at the mouth of the jet, while he is lighting the gas with a paper, and

* This proportion ought especially to be observed when the *oxygen* gas has been obtained from the *oxide of manganese*, and the *hydrogen* gas from the decomposition of *water* by *iron filings* acted upon by *sulphuric acid*.

which is an infallible sign of the excellence of his gaseous mixture for all experiments when the highest temperature is required. The stoutest *platinum* wire will run before it into rapid fusion, and is immediately exposed to combustion. Pure *magnesia* may thus be easily fused, and be made to form a frit, which cuts glass like a diamond. The edges of *Iceland spar*, that most refractory of all bodies, when exposed in thin laminæ, are by the flame of this mixture invested with a white opaque enamel. Rubies and sapphires become liquid, and flow together into one boiling mass. Grains of *iridium* melt and burn like *platinum*. Under these circumstances, if the earth of *barytes* be now exposed to the powerful heat of the burning gaseous mixture, unless the valve be partially opened, so as to allow a very small body of flame, like the point of a fine needle, to act upon it, it will be converted into a greenish glass, or into a substance externally resembling horn, disclosing no metallic lustre to the action of the file. In my first experiment this year for exhibiting the metallic base of *barytes*, the attempt entirely failed; but I was thereby guided, for the first time, to the cause of the failure; for the *barytes* which failed was taken out of the same bottle as that which afterwards exhibited, in so imminent a degree, the *metallic* lustre I have already described. The cause was owing to the length of time in which fusion had been going on, and the too powerful heat to which the revived metal had been exposed: it came away altogether in white fumes, settling upon the forceps, and tinging the flame with that olive-green colour which always characterizes the combustion of the metallic base of *barytes*. The same would happen with *silver*; the whole of the metal would be dissipated in white fumes, which would settle upon the supporter if the heat be continued long enough, and the flame also is in this experiment tinged of a paler greenish hue. For exhibiting, therefore, the metallic lustre of *plutonium*, call it a *metal*, or a *protoxide*, or by whatsoever other name chemists may hereafter decide, it is necessary that the experiment should be conducted with care. The *barytic earth* should be, in the first place, rendered as perfectly an *anhydrate* as possible; the presence of an atom of water would inevitably cause the experiment to fail. Of course it is hardly necessary to add, that water ought by no means to be used as a substitute for oil in the safety cylinder; for this would again give rise to failure. Having a small cake of the hard *anhydrate* of *barytes*, supported by a pair of forceps, allow the flame gradually to act upon it, until it exhibits after fusion the deep jet-black substance I have before alluded to. Then while it is hot, rub one of the prominent points of this black substance upon the sharpest and finest file, previously made warm before the fire, taking care not to cut it too deep, because that would disclose the *barytic earth* yet remaining below the part which has been fused. The *metallic*

lustre ought to equal that which is presented after filing the end of a piece of iron wire, or the experiment has failed.

It has always failed when the fused *barytes* leaves traces of a white powder upon the file.

It has, moreover, always failed when the fused mass exhibits the slightest degree of translucency; or is of a grey, greenish, or white colour.

It has always failed when the experiment has been protracted beyond the point at which the metal begins to burn, and is dissipated in white fumes.

To all those gentlemen who have so often witnessed the perfect *metallic* appearance of the melted *barytes* in my lecture room at *Cambridge*, I may now appeal for the truth of the result; being very anxious that others should also witness the same appearance, to whose testimony it may never be in my power to appeal. It would be to no purpose enumerating the names of those who have been present with me during my experiments; persons not only of this University, but from various parts of the kingdom, visiting *Cambridge*, have satisfied themselves in this respect. Upon *April 10, 1819*, there were present for this purpose from *London*, Mr. *W. Hamilton*, Under Secretary of State; our Envoy to Constantinople; Mr. *B. Frere*; his brother Mr. *Serjeant Frere*, Master of Downing College; Mr. *Meyer*, Consul at *Corfu*; Mr. *Mackenzie*, of the Foreign Office; and also Colonel *Leakes*, of the Artillery; all of whom, independently of those whose residence here gives them more frequent opportunities of being present at these experiments, are able to vouch for the truth of the statement I have made.

But so lately as the 25th of *April*, of the present year, *Prof. Müller*, of *Copenhagen*, together with several gentlemen of this University, were present at a series of experiments conducted with the *gas blowpipe*, and all of them witnessed the revival of the *metal of barytes* by means of this instrument. The experiment was conducted as before described; and was repeated in their presence always with the same result. *Prof. Müller* took away with him a specimen of the metal so revived; but which of course would speedily afterwards be again converted into the state of an oxide. It is sufficient, however, to observe, that they all saw it in the metallic state. If, therefore, the experiments elsewhere should not be attended with the same results, the circumstance must admit of this obvious inference; that the means resorted to, and the manner of using the instrument, have not been made adequate to the end proposed, which otherwise might have been accomplished.

The other chemical changes which bodies undergo when exposed to the full powers of the *gas blowpipe*, and which appear to me to be of a nature sufficiently remarkable to merit the attention of chemists, are as follow:

1. *Rock crystal*, or any other highly refractory *siliceous* or *aluminous* substance, being allowed to fall upon a deal board, while in fusion, and there left to become cool, exhibits upon the surface that came into contact with the wood, where it forms a charcoal cavity, a *metallic* lustre, equal to that of polished silver. This *metallic* appearance may be preserved for any length of time unaltered. Some of the students in this University have preserved specimens of melted *rock crystal* thus invested by a seeming *metallic* body, the nature of which is unknown.

2. *White opaque quartz* melted in the flame of the *gas blowpipe* becomes so highly limpid and transparent, that after fusion it has all the appearance of *rock crystal*. The *Plutonists* may, perhaps, hence infer, that the transparency of *rock crystal* is owing to the heat it has sustained.

3. The varied and striking colours of *metallic oxides* are surprizingly developed by the flame of this blowpipe. Among these, one of the most striking from its beauty is the intense red, or bright purple, colour of the *oxide of calcium*, as displayed during the fusion of *Iceland spar*. It is a colour which cannot easily be described; being much more vivid and striking than the colour which the *oxide of strontium* communicates to the same flame, and of a livelier hue. Add also the beautiful *rosy* colour of the *oxide of gold*, after its combustion upon pipe-clay; the deeper purple hue of the compound *oxides of gold and tin*, when the alloy obtained from the *precipitate of cassius* has similarly sustained combustion, the green colour of the *oxide of rhodium*, &c. &c.

4. The gaseous fluid which escapes during the fusion of pure *silica*, has never been collected, and, of course, remains unknown.

5. The dark powder dispersed during the combustion of *platinum* requires further examination. From some experiments made with this substance, of which I before published an account,* it seems to be the *protoxide* of the metal.

6. The dark sooty-looking powder that remains after the solution of crude *platina* in *nitromuriatic acid*, divested of all metallic particles which may remain undissolved, and consisting essentially of the oxides of *iridium* and *osmium*, is one of the most refractory substances that have been exposed to the *gas blowpipe*. By making a filter which was invested with this powder into a pellet, and exposing it upon charcoal to the full power of the flame, I succeeded in melting it into a brittle *metallic* mass mixed with a deep blue glass, which had resulted from the *silicated alkali* contained in the paper of the filter, as in all vegetable bodies. It was not rendered malleable. The surface of the charcoal during this experiment was covered with an oxide of a dark lilac purple hue.

* See *Annals of Philosophy*.

7. The beautiful iridescent crystals which form in the aqueous solution of the *muriates* obtained by dissolving crude *platina* in *nitromuriatic acid*; and after the precipitation of *platinum* by *muriate of ammonia*, are easily reduced to the metallic state upon charcoal by the flame of the *gas blowpipe*. The metal so obtained is perfectly malleable, and has all the characters of *platinum*; excepting that it is almost insoluble in *nitromuriatic acid*, even when this acid is in a boiling state; owing, probably, to a small portion of *iridium* with which the *platinum* is contaminated.

8. The *prussiate of palladium* obtained by the precipitation of this metal by *prussiate of mercury*, may be decomposed, and the pure *palladium* revived upon charcoal, with all the whiteness of silver, and made perfectly malleable. This last experiment with the *gas blowpipe* is remarkable for the beautiful sapphire-blue colour with which the flame is tinged during the revival of the *palladium*. It affords, moreover, one of the easiest processes for obtaining small specimens of pure *palladium* to which the chemist can have recourse. All that is necessary is briefly this: Dissolve crude *platina* in *nitromuriatic acid*; evaporate to dryness; add distilled water; precipitate the *platinum* by *muriate of ammonia*; filter the supernatant solution; precipitate *prussiate of palladium* by *prussiate of mercury*, and collect the precipitate upon a filter. Now make this filter containing the precipitate into a pellet, and expose it upon charcoal to the flame of the *gas blowpipe*. The result will be a malleable bead of the purest *palladium*, coated over with blue glass from the *silicated alkali* of the paper filter, which separates from the metal upon the first blow of a hammer. Glass will also appear in all experiments for the revival of metals where filters have been thus used. It is, perhaps, a similar result to the glass which remains after the combustion of bank-paper notes.

9. The revival of the *salts* and *oxides* of the four metals obtained from arenaceous *platinum*; namely, *iridium*, *osmium*, *palladium*, and *rhodium*, whether as alloys, or as pure metals, afford some of the most curious and amusing experiments with the *gas blowpipe*. The last experiment with these metals which I shall now describe relates to the revival of *rhodium* from its oxide, as obtained in the following manner: After the solution of crude *platina* in *nitromuriatic acid*, having precipitated *platinum* by *muriate of ammonia*, and *palladium* by *prussiate of mercury*; also *iron* by *prussiate of potass*, immerse a clean plate of *zinc* into the filtered liquor, and leave it for several hours. A precipitate will fall of a brownish-red colour, which, when dry, is black. Before the *gas blowpipe*, the filter, containing this precipitate, exhibits a sort of coruscation, like a little firework; but at length a slag will remain on the charcoal of an iron-brown colour, containing within it a bead of a silvery-white metal. This metal is harder than wrought iron. It was

also brittle. I endeavoured to dissolve the slag containing it by boiling it in caustic potass, but could not succeed. Afterwards I added *nitromuriatic acid*, and having evaporated the acid to dryness, there remained a rose-coloured salt. Still the bead of metal remained chiefly undissolved. I then fused it with *sulphur*, and expelled the sulphur on *charcoal* by the common blowpipe. Still the metal was brittle. I then tried to cupel it upon pipe-clay with borax; it was infusible; but it communicated a *brown*, and afterwards a *black* colour to the borax; which are the hues of the *deutoxide* and *protoxide* of *rhodium*. The metal also in these experiments always became *black* by heat; which is another character of *rhodium*. By mixing together dry *caustic potass* and *borax*, in equal parts, I found I could hold it in perfect fusion upon *charcoal* before the common blowpipe, and distinctly discern the minute globules of metal separating in the boiling flux. Still the result, when cold, was brittle, and quite *black*. I then boiled it again in *nitromuriatic acid*, and by evaporation to dryness had a beautiful *rosy muriate*. Afterwards I added the acid once more, in a dilute state, with *common salt*. The *rosy soda muriate* appeared as it became dry. I washed the residue repeatedly with *alcohol*, and dried it. Distilled water was then added, and the solution, by gentle heat, being evaporated to dryness, the *rosy-coloured soda-muriate* appeared as before; but in less quantity. These experiments were conducted upon a bead of metallic *rhodium*, revived from its oxide before the *gas blowpipe*, the weight of which originally could hardly have equalled 1-10th of a grain. When the *oxide* has been obtained in greater quantity, nothing more is necessary for its revival, than to mix it with *oil*, and expose it upon *charcoal* before the flame, which should be made to act vertically upon the *oxide* by means of a bent tube.

The experiment is beautiful, owing to its simplicity, to the facility with which it may be conducted, and to the curious result which follows it; namely, the revival of metallic *rhodium*, exhibiting all the whiteness and lustre of the purest silver, and being perfectly malleable. This experiment was attended with more than usual success on *Saturday, May 12*, when it was publicly performed in my lecture room in *Cambridge*. The black oxide of *rhodium*, precipitated from its solution by *zinc*, was mixed in the form of powder with *oil*, and exposed, upon *charcoal*, to the flame of the *gas blowpipe*. It was first melted into a black slag; then into a brittle white regulus, which, by continuance of the heat, became a bead of metal as white as the purest silver. It then began to burn, like *platinum*, with scintillation; and the flame of the burning metal was tinged with a fine *emerald-green* colour; proving that one of the oxides of *rhodium* may have the same hue. The bead of metal was then placed upon an anvil, where it sustained the shocks of a large hammer; and was finally extended and flattened in its form. It

is, therefore, evident that *rhodium* may be rolled and made into foil like *platinum*. But there is one curious circumstance which will render it always brittle. If a quantity of the *soda-muriate* be mixed with the *black oxide*, a regulus may be revived; but no degree of heat, nor any repetition of fusion, will render it malleable. A portion of it will then always be converted into glass, and it will exhibit a granular texture, depriving it of malleability. Nor is the *soda-muriate* itself so likely to be revived in a malleable state as the *black oxide* of the metal, which never fails.

10. The hard *carbonate of magnesia* from the East Indies, described by Dr. Henry, in a late number of the *Annals of Philosophy*, fused before the *gas blowpipe* into a white enamel, communicates a purple colour to the flame.

11. A remarkable difference may be observed in the fusion of the several crystallized and amorphous varieties of the phosphates of lime. Some, like the white opaque *apatite*, from *Devonshire*, fusing into a jet-black gloss which is magnetic; others, as the conchoidal *apatite* of *Modum*, in *Norway*, and the earthy *apatite* of *Estremadura*, fusing into greenish and limpid glasses, which have no magnetic properties.

To conclude all these observations upon the *gas blowpipe*, it will be useful to some of your readers to be informed of the best method of preserving the bladders employed for containing the gaseous mixture; because it is difficult to meet with very large bladders, and they are soon rendered unfit for use without the following precaution. Let them be kept carefully rubbed over with oil, and distended with common air, when not wanted for experiments. By attending to this mode of preserving them, we have found that the same bladder may be made to last in constant use for the *gas blowpipe*, upwards of two years, without becoming porous, which so frequently happens where the oil has not been applied, in consequence of the attacks of insects.

EDWARD DANIEL CLARKE.

ARTICLE IV.

Reply to Dr. Yelloly's Remarks on the Estimate of Mortality from the Operation of Lithotomy. By W. Prout, MD. FRS.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

May 9, 1821.

IN reply to Dr. Yelloly's remarks on my estimate of mortality from the operation of lithotomy in your last number, I beg leave to observe that the adoption of the ratio alluded to is not the result of inadvertency, but design. My object is to give a

mean which, in the present improved state of surgery shall tolerably well serve for the whole kingdom. The data we possess are obviously too imperfect to admit of this being done with much precision, and the ratio actually deducible from them (viz. 1 in about 6 $\frac{1}{2}$) seems, when all the circumstances are duly considered, to exceed the truth. On the other hand, the ratio deducible on general principles from the same data, though of course not necessarily more correct than the actual one, happens to coincide more nearly with my views; hence (and as I am also reasoning *generally*), I give it the preference.*

The circumstance which renders the data in question particularly defective is their not including the metropolis, where, according to Mr. Smith's estimate, two-fifths of the whole number of operations in the kingdom are performed. We have no means of obtaining correct information on this point, which is much to be regretted, but when we take into account the acknowledged skill of our surgeons, and the fact that nearly one-half of the above proportion of cases are private, in which of course every circumstance that united extraordinary skill and attention can devise to ensure success is attended to, we cannot hesitate, I think, to draw the conclusion, that the rate of mortality is as favourable, if not more so, in London, as in any part of the country. This circumstance, if admitted, will tend to diminish considerably the general ratio of mortality; perhaps, reduce it to 1 in 7, or upwards, even according to the usual mode of estimation by taking the average of a number of past years. But this mode of estimation is not exactly applicable to my purpose. I wish to represent the rate of mortality as it exists *at this time*, when I am willing to believe the surgical art is more perfect than at any former period: this, if admitted, will allow a still further reduction in the rate of mortality; so that upon the whole, I trust the ratio given does not much exceed the truth; and if it does, I acknowledge that I feel much less regret in falling into an error on this side of the question than the other.†

Such are the principal reasons which induce me to prefer the general ratio I have given to that actually resulting from my imperfect data. I do not mean to assert that they are absolutely conclusive, but, on the contrary, willingly admit that there is room for diversity of opinion on the subject. Every one, therefore, must decide for himself.

* *Means*, generally speaking, are correct in proportion to the number of observations. Thus, from the multiplied observations of the Bristol data, the mean deduced on general principles differs very little from the actual mean, though both are evidently very far below the general mean as applicable to the whole kingdom. On the other hand, the mean, as deduced in general principles from the Norfolk data, happens to considerably exceed the actual mean, the periods of observation being only two (viz. *before and after puberty*). This error in excess, however, though of a different kind, serves, in the final generalization, to counteract the error of deficiency in the Bristol data.

† Dr. Marcet estimates the mortality in Guy's Hospital at about 1 in 7. Mr. Martineau, of Norwich, has, I understand, during the last 17 years, performed the operation 65 times with the loss of only *two* patients.

From the summary nature of my little volume the above has been omitted, as well as a great deal more on some other points, which I fear will in consequence be liable to be misunderstood. I hope, however, to have it in my power to obviate this objection at no very remote period.

I am, yours truly,

WILLIAM PROUT.

ARTICLE V.

Upon a new Hydro-Pneumatic Blowpipe, so constructed as to maintain during two Hours, uninterruptedly, a Degree of Heat capable of melting Platinum; and this by propelling the Flame of a small Wax Taper with Atmospheric Air. By Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, Member of the Royal Academy of Sciences at Berlin, &c. &c.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Cambridge, May 15, 1821.

To the various successive improvements which the *blowpipe* has received, we may now apparently add another, possessed of considerable advantages:—I allude to that form of the instrument wherein the air is propelled from a jet by the pressure of a quantity of water; this being the kind of *blowpipe* on which these improvements are founded.

The advantages of the old instrument consisted in the operator having both his hands at liberty; and in the relief which it afforded from that fatigue and possibility of injury to the lungs incident to a protracted restraint on their free action, to which persons using the common mouth blowpipe were liable. To these advantages, which the new instrument also possesses, we may add the following:

1. Either common air, or any other gaseous fluid may be used for the propelling current, by condensing it in the reservoir, and thus experiments may be made on the fusing powers of the different gases with perfect ease and convenience.
2. The power of entire exhaustion possessed by the new instrument, ensures the operator from any admixture of common air, where *oxygen gas*, or any other gaseous fluid is to be employed.
3. The old instrument, although very useful for bending tubes, or other ordinary purposes, required to be repeatedly restored to action by fresh supplies of air, at intervals seldom exceeding five minutes, in the common-sized instruments. In the new instrument there is this great improvement; that a steady flame of

two hours continuance may be maintained, of the most perfect shape and uniform temperature, uninterrupted by casual currents from the pneumatic reservoir.

4. The troublesome interruptions caused by the ejection of water, while supplying the apparatus with air, which were common in the old instrument, do not happen in the new one.

5. The new instrument may remain unemployed for any length of time, being always ready for instantaneous use, and requiring no other preparation than merely that of lighting the wax taper employed to supply the flame.

The manner in which this instrument was brought to its present state of perfection, affords an anecdote which may not be uninteresting to your readers; because it will show the force of mechanical skill as it is sometimes remarkably conspicuous in uneducated minds. A servant of mine, who has been frequently employed in attendance during my lectures in mineralogy, seeing me reject the old instrument as unfit for the uses to which I wished to apply it, asked me the reason of my setting it aside. To this I answered that the short space of time in which it continued to propel the flame rendered it inconvenient; and that I would rather use the common mouth blowpipe than be liable to such frequent interruption from the necessity of supplying it every five minutes with fresh air. This caused him to inspect the inside of the instrument; when he simply observed, "*It is very awkwardly contrived; I could make a better myself!*" and in good earnest, without further communication with me upon the subject, he fell to work, and produced the new improved apparatus, which it is my present purpose to describe, and to make as generally known as possible. The inventor's name is *Johnson Tofts*. Upon the principle which he has adopted for the improvement of his blowpipe, such instruments are now manufactured in London. By means of one of these instruments, *gold* might be exhibited in a state of continual fusion for almost any length of time.

A trial has been made of the use and powers of this improved blowpipe throughout an entire course of public lectures in mineralogy before the University, with such success, as to produce sufficient proof of its convenience and efficacy. The effects witnessed when *oxygen gas* was employed exceeded those usually produced by the same agent from a gasometer; owing to the condensed state in which the gas was propelled; while the cone of flame was much more manageable. *Platinum* wire of some thickness was fused; *platinum* foil offered no resistance whatever. The *steel* mainspring of a watch underwent brilliant combustion; and even when the instrument is charged with common atmospheric air, thin cuttings of *platinum* foil sustain an instantaneous fusion.

Had we possessed this apparatus before the *gas blowpipe* was invented, many of the results obtained by that powerful instru-

ment would have been anticipated. It is not to be expected, however, that the fusing powers of the two blowpipes can be compared together; but from the safety of *Toft's blowpipe*, a child may use it; whereas the other would be indeed a dangerous toy.

In the old apparatus, when the instrument is charged, the space E C G H is occupied by air, and the water rises to the dotted line C D. When it has been kept in action until the water has its surface at the dotted line A B, the action ceases, and the space E A F C must always remain filled with common air.

In the improved hydro-pneumatic apparatus, which I have called *Toft's blowpipe*,

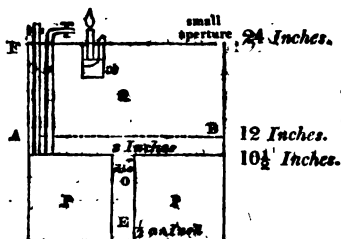
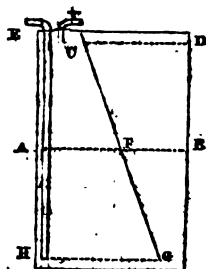
when the instrument has been charged for use, P P is filled with air, and the water remains entirely above it in the vessel Q, whence it will descend through the cylinder O E so long as any air shall remain in the reservoir P P, and the water will entirely fill this reservoir; so that whatever air it may be desirable next to introduce will again displace the water, and drive it up to Q without admixture of common air.

All gases are introduced by means of a bladder and a condensing syringe, which screws on to the stop-cock F. And it has been found better to introduce atmospheric air with a syringe than to fill the reservoir with air from the lungs; also to supply the machine with water, or to remove it by means of a common siphon.

The instrument is so simple that a more minute description of it is unnecessary. Upon these principles it is manufactured and sold by Mr. Newman, maker of philosophical apparatus, in *Lister-street, Leicester-square*. The usual size of one of these blowpipes is two feet high, two feet long, and five inches wide. They are made either of copper or tin, and enclosed in a wooden case, which serves as a table, and a rest for the arms; the wax taper, being sunk into a cylinder at X, is elevated or depressed by means of a screw and a rack. But a stationary spirit-lamp, if it should be preferred, may be fixed in the same place.

I remain, Sir, yours, &c.

EDWARD DANIEL CLARKE.



ARTICLE VI.

On Oil and Coal Gas.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Sheffield, May 12, 1821.

I HOPE you will indulge me with the insertion of a short reply to Mr. Ricardo's paper on Oil and Coal Gas, which appeared in your last Number. I would not have troubled you with a second paper on this subject, had I not thought it a duty imposed upon me to correct the erroneous statement which Mr. R. made in the postscript to his last paper respecting the price of coal gas in Sheffield.

If I had allowed his remarks to have passed unnoticed, he and your readers would be justified in drawing the conclusion, that my first paper was incorrect, and that the consumers of gas in Sheffield were imposed upon by the Gas Light Company, by paying a much higher price for gas than what the Company professed to charge them.

Mr. R.'s first paper has been so completely answered, respecting the comparative illuminating power of the two gases in question, by Mr. Lowe, of Derby, in the *Philosophical Magazine* for the last month, that any further remarks on that head are rendered unnecessary.

Mr. R. still persists in his assertion that the London Chartered Company possess advantages superior to any other, although they only divide 8 per cent. and yet other Companies have divided 10 per cent. The Sheffield Gas Works are generally allowed to be as well built, and as completely and conveniently fitted up, as those of any other place; therefore, we may fairly presume that the capital employed in the buildings, &c. will be nearly upon a par with that of the Chartered Company, in proportion to the size of the works and the quantity of gas required. To what then must the *advantages* be attributed in those *places* (for every one, except Mr. R. I think, will allow that they have *real advantages*) where the Companies make *greater profits*, and sell their gas *cheaper* than at the Chartered Company's Works! It must be either owing to superior management, and the economy used in that management, or to the cheapness of the destructible articles (retorts, coal, &c.) used in the manufacturing of gas. As Mr. R. is not willing to allow that the cheapness of coal, labour, &c. is of much benefit to a gas light manufactory, though I entirely differ with him on that point, it must be in the economical management that some provincial towns have the advantage over the Chartered Company. Perhaps Mr. R. will

not be willing to allow this point. However, certain it is, that several Gas Light Companies *have advantages which the Chartered Company has not, or they could not make greater dividends, and sell their gas for a less price.* I admit that it is *one great advantage* to a Gas Light Company to have the greatest number of lights in the smallest compass; but I have been informed by an *able and experienced engineer*, that the expense of the main pipes in London is as great upon the average, for the same number of lights upon them, as in most of the large country towns. In calculating the price of gas from the rate cards of the different towns, Mr. R. assumes that all burners denominated No. 3, Argand, are of the same size, and consume the same quantity of gas. From this supposition arises, in a great measure, the erroneous statement in his last paper respecting the price of gas in Sheffield. The No. 1, Argand, used in Sheffield, consumes three cubic feet per hour, by Mr. Cleg's, meter; the No. 2 consumes $4\frac{1}{2}$ cubic feet per hour; and the No. 3 consumes six cubic feet per hour. I have seen a No. 3, Argand, from Birmingham, tried by the same meter against a No. 2, Argand, such as are used in Sheffield, and the difference in the quantity of gas consumed by the two burners was only half a cubic foot per hour. I likewise, by the same meter, have seen a No. 3, Argand, with 24 jets, consume seven feet per hour. Very few No. 3, Argand burners, are used in Sheffield, owing to the No. 2 being considered nearly equal to the No. 3, in other places. The average time of lighting through the year cannot be calculated at a later hour than six o'clock, as that is the average time of the setting of the sun; and we may frequently see the shops lighted up in winter an hour before the time of sunset. Mr. R. only calculates $3\frac{1}{2}$ hours' burning, till 10 o'clock, which is certainly half an hour *at least* too little. The Sheffield card is calculated from six o'clock upon the average; and as the No. 3, Argand, consumes six feet per hour; in 313 evenings till eight o'clock it will consume 3756 cubic feet of gas, which at 12s. per 1000 feet amounts to 2l. 5s. 0 $\frac{3}{4}$ d.: the charge in the rate card is 2l. 5s. per annum, and yet Mr. R. by his ingenious mode of calculation, says, that the cost price of gas in Sheffield is 17s. per 1000 feet. The charge for the same burner till nine o'clock is 3l. 9s. and till 10 o'clock, 4l. 13s.; the last two charges are rather more than at the rate of 12s. per 1000 feet; but if the rental amount to 20l. a discount of 20 per cent. is allowed, which reduces the gas to less than 10s. per 1000 feet. In manufactories where the meter is used, the regular charge is 12s. per 1000 feet, with a discount of from 5 to 20 per cent. in proportion to the rental, but all rentals of or above 20l. are allowed 20 per cent. I am informed that the annual rentals for gas in several of the silver-plated manufactories are 50l. and upwards. Gas is found to be so much superior to oil for solder-

ing silver-plated goods, that it is generally used for that purpose in these silver manufactories where it is introduced.

I hope this explanation will convince your readers that my first statement was not so incorrect as Mr. R. wished to make it appear.

I remain, yours respectfully,

A SUBSCRIBER.

ARTICLE VII.

Researches on the Composition of the Prussiates, or ferruginous Hydrocyanates. By J. Berzelius.

(Continued from p. 308.)

IN order to examine whether, in these experiments, the remaining bases be in the state of common carbonate, I heated a mixture of carbonate of potash with six times its weight of oxide of copper in a proper apparatus for receiving the gas that might be disengaged. As soon as the mixture had acquired a red heat, carbonic acid gas began to be given off, and continued for more than half an hour. Thus the oxide of copper has the property of expelling the carbonic acid at a high temperature, and of forming a kind of double salt, in which it is to be presumed that one-fourth of the potash is combined with the oxide of copper, and three-fourths with the carbonic acid. This salt is decomposed in the humid way, the water seizes a mixture of caustic potash, and carbonate of potash, and the oxide of copper remains undissolved. This last phenomenon also ensues when oxide of copper is dissolved in hydrate of potash by igneous fusion. The compound, which in the liquid state is transparent, and of a yellow colour, is decomposed by water, which takes the potash, without, at the same time, dissolving the oxide of copper, with which it was combined.

It was necessary, therefore, in order to obtain a more decisive result, to analyze a ferruginous prussiate whose base is incapable of retaining the carbonic acid, and I chose for this purpose that of lead.

12.36 grains of prussiate of lead were mixed with 308.8 grains of oxide of copper obtained by calcining copper in a cupelling furnace. The gas was collected over mercury in a graduated receiver, in which it could be accurately measured. To determine with the greatest possible accuracy the relative volume of carbonic acid gas to that of the azote, a portion of the gas collected towards the end of the operation, and consequently free from atmospheric air, was examined separately: 136 mea-

measures placed in contact with caustic potash were reduced to 45.4 measures; but $45.4 \times 3 = 136.2$; which gives, as exactly as can be expected in experiments of this nature, two volumes of carbonic acid gas for one of azote. Thus this experiment proves that the carbon and the azote, in these salts, are in the same proportions as in cyanogen. No water was formed, except a slight and unimportant trace of moisture.* The whole quantity of gas obtained consisted of 102 volumes of carbonic acid and 51 of azote; the former weighed 3.1 grains, and the azote 0.99 gr. which give for 100 parts of salt, 11.05 of carbon, and 12.84 of azote; or together, 23.89 of cyanogen. This quantity, added to the other element of the salt employed, exceeds its weight by 6.19, if the bases exist in it in the state of oxide; but if the prussiate be composed of one atom of cyanuret of iron with two atoms of cyanuret of lead, the sum of the cyanogen, iron, and lead, obtained, gives almost exactly its weight.

It appeared to me very easy to verify this idea by acting on the ferruginous prussiate of lead by sulphuretted hydrogen gas: 18.84 grains of the anhydrous salt were introduced into a small weighed glass ball, blown by the lamp. I caused a current of sulphuretted hydrogen gas to pass through the ball, which, on quitting it, traversed a tube filled with muriate of lime. While cold, the prussiate of lead was not altered by the gas, but on heating the ball by a spirit-lamp, it was immediately blackened, and hydrocyanic acid evaporated with the sulphuretted hydrogen that passed off in excess. No trace of moisture was perceived during the operation, not even when the mass in the ball was red-hot in the sulphuretted hydrogen gas. The remaining mass weighed 17.06 grs. and was composed of sulphuret of iron, at the minimum, and sulphuret of lead. The tube containing the muriate of lime had only gained 0.0077 of a grain in weight, a part of which was sulphur. Thus this experiment proves that in the anhydrous ferruginous prussiate of lead the metals are not in the state of oxide; for even if we suppose in this experiment that no water is obtained, in consequence of the sulphur seizing the oxygen of the metallic oxides, the sulphuric acid, so produced, ought to have combined with a part of the oxides, and have occasioned a much more considerable increase of weight than was obtained in the experiment; on the other hand, if the salt be really composed of one atom of cyanuret of iron with two atoms of cyanuret of lead, its decomposition by sulphuretted hydrogen should give for 18.84 grains of metallic cyanuret, 17 grains of sulphuret; which agrees, as nearly as possible, with the result of the experiment. The composition then will be

* The original has been shortened.—*Edit.*

According to experiment.		According to calculation.*	
Iron	8.81	8.68	678.43
Lead	65.91	66.18	5178.00
Carbon	11.05	11.55	1967.52
Azote	12.84	13.59	
	<hr/> 98.61	<hr/> 100.00	<hr/> 7823.95

Thus the result of analysis approaches that by calculation as near as can be expected in an operation of so complicated a nature.

If we apply this to the salt with base of potash, we get the following result: the relation of the volume of the carbonic acid gas, found by experiment, was to that of the azote as 3 : 2, and the weight of the carbonic acid gas obtained from 7.72 grs. of salt was 4.13 grs.: if we add one-third the quantity necessary to make the volume of carbonic acid twice that of the azote, the weight of the carbonic acid will be rather more than 5.5 grains, or 11 for 100 grains of the salt; but if the ferruginous prussiate of potash be composed of one atom of cyanuret of iron and two atoms of cyanuret of potassium, 100 grs. will give 11.10 grs. of carbonic acid gas, a difference within the limits of inaccurate observation.

The composition of this prussiate, in the state of crystals, calculated after the formula, $\frac{1}{2}\text{Fe Cy}^2 + 2\text{K Cy}^2 + 6\text{Aq}$, gives per cent.

Iron	12.85	= protoxide of iron,	16.54
Potassium.	37.11	= potash.....	44.68
Cyanogen.	37.22		
Water	12.82		

I think it useless to give the result of the calculation of the two other salts analyzed. They accord with the following formulæ: $\text{Fe Cy}^2 + 2\text{Ba Cy}^2 + 12\text{Aq}$; and $\text{Fe Cy}^2 + 2\text{Ca Cy}^2 + 24\text{Aq}$. But we must remember that, although in a state of efflorescence, these two salts retain an atom of water, for some reason not very easily explained.

These experiments prove that the salts called prussiates, or ferruginous hydrocyanates, are in fact cyanurets, composed of one atom of cyanuret of iron and two atoms of cyanuret of the other metal.

What is the nature of these compounds when they contain water? It is more difficult to answer this question than at first it seems to be. In the ferruginous prussiates of potash and lead, there is exactly the quantity of water necessary to transform them into hydrocyanates, whence they may be considered

* According to the formula, $\text{Fe Cy}^2 + 2\text{Pb Cy}^2$.

§ I use the sign Cy for cyanogen instead of N C², to avoid complex numbers.

as double hydrocyanates. But are their elements really combined in this manner, or may they be regarded also as cyanates with water of crystallization?

In compound substances, whose elements may be conceived to be united in several ways, is it indifferent in what manner we imagine the union to take place? Are the products of their decomposition determined solely by the decomposing power? A multitude of circumstances concur in favour of this idea. But if it were correct, a compound body, whose elements are capable of combining at high temperatures, in such a manner as to afford other compounds, endued with a certain tension, at the common atmospheric temperature and pressures, I say this substance must continually decompose in the air, till the tension of its elements is balanced; and in a vacuum it cannot exist at all. It appears to us that nitrate of ammonia would decompose in this way in a vacuum, if it were a matter of indifference whether we consider it as composed of an atom of ammonia, an atom of nitric acid, and an atom of water, or of an atom of protoxide of azote and two atoms of water, since, in a vacuum, there is no obstacle to prevent these bodies obeying the influence of their tension, as well as the preponderating affinities, any more than at a high temperature, when those two substances are actually formed. It is probable, therefore, that the elements of nitrate of ammonia are combined rather as nitric acid, ammonia, and water, than as protoxide of azote and water. In like manner, when a salt effloresces at the common temperature of the atmosphere, we consider the water as existing as such in the salt, but retained by so weak an affinity, that it is overcome by its tension.

Fulminating gold and silver explode by very slight elevation of temperature, and water results; but these substances are permanent in a vacuum. The hydrogen and oxygen which they retain with so weak an affinity were not, therefore, in the state of water, but combined in another manner, and it is only in the act of explosion that water is formed. But if in these substances, whose composition depends on so weak an affinity, water be not produced, when the effect of its tension is facilitated by a vacuum, we may presume that all bodies which exhale aqueous vapours in a vacuum must contain water ready formed, and that they retain it by a very weak affinity.

I put crystals of ferruginous prussiate of potash into a vacuum with sulphuric acid; at the temperature of 35.4° Fahr. they lost all their water of combination: a small piece of a crystal introduced into the vacuum of a barometer made it fall 0.005 in. at the temperature of 59° Fahr. I do not give this, however, as a very accurate result, from the extreme difficulty of making a solid body rise through the mercury, perfectly free from air adhering mechanically to its surface. The experiment proves, however, that the water of crystallization in this salt is endued

with a certain tension, which varies with the temperature. It appears, therefore, that the salt in question contains water ready formed, and not hydrocyanic acid, and oxidated bases; for it is not very likely that the base and the acid should be decomposed and recombined, in proportion as the tension of the water varies, according to the temperature and the quantity of aqueous vapour in the surrounding atmosphere. In this case, we must consider the crystallized ferruginous prussiate of potash, as a double cyanuret of iron and potassium, combined with water of crystallization; after the manner of salts, all the external characters of which it possesses even in its efflorescent state.

III. On the Hydrocyanate of Iron and Ammonia.

In order to learn the nature of the ferruginous hydrocyanates with more certainty, I also examined the composition of the ammoniacal one. It appeared to me at first probable that it might throw light upon the true nature of ammonia. If, for example, this salt were reducible to cyanuret of iron and the metallic substance which amalgamates with mercury, when ammonia is decomposed by the electric pile, and if it were possible to obtain this salt free from water, or, what is the same, without oxygen, it is evident that, in this case, it would be a double cyanuret, and it would be easy to determine whether the metallic radical of ammonia is composed of an atom of azote and of six atoms of hydrogen, as appears to result from the experiments of MM. Thenard and Gay-Lussac, or whether it consists of an atom of nitricum and six atoms of hydrogen, according to the conjecture which I have hazarded. In the first case, this salt would appear to be composed of cyanuret of iron and hydrocyanate of ammonia; and the hydrogen assigned to the hydrocyanic acid would in fact be the additional atom of hydrogen required to metallize the ammonia. In the second case, on the contrary, there would be no other hydrogen than that which is a constituent part of the ammonia. But there is still a third case: it is that this combination might consist of hydrocyanate of protoxide of iron and hydrocyanate of ammonia, and which would not part with the oxygen and hydrogen necessary to the existence of the hydrocyanate, without being at the same time totally decomposed; and unfortunately this is the actual state of the case.

The experiments necessary to determine which of these cases happens, are neither difficult, nor subject to give ambiguous results. The hydrocyanate must contain four atoms of ammonia and one atom of iron: since the oxygen presupposed in this alkali must be double that which iron absorbs in becoming protoxide. Consequently, in the first case, the salt decomposed by heat ought to leave 27.53 per cent. of its weight of red oxide of iron; in the second, 31.18; and on the third, 25.9. The greatest difficulty in this experiment was that of obtaining the

ammoniacal salt pure, because the prussian blue of commerce affords it much mixed with other substances, and during evaporation with them, it constantly decomposes. I, therefore, prepared some cyanuret of iron and lead, which I decomposed by caustic ammonia. The solution was evaporated in *vacuo*, and having powdered the salt, I again dried it in *vacuo* to separate all the water mechanically mixed with it.

In order to determine whether this salt contains any water, I heated it in a small retort made with the lamp, and received the products of the distillation in a long tube, that by the difference of their volatility they might be deposited at different distances from the heated part. The first effect produced by the heat is that of rendering the salt green at the bottom of the retort, and the smell of hydrocyanate of ammonia is perceived at the open end of the tube. The greenish colour was soon succeeded by a greyish-yellow, considerably resembling that of the pulverized salt before it is heated. The yellowish stratum increased in thickness, and at the same time a very minute greenish stratum which separated it from the unchanged salt gradually rose towards the surface, where it at last disappeared, leaving the salt completely converted into a yellowish-grey mass. During this decomposition, water continued forming and depositing in small drops in those parts of the tube which were near the retort. A great quantity of hydrocyanate of ammonia was at the same time disengaged, and when the salt had driven the atmospheric air out of the apparatus, it began to crystallize at some distance from the water, so that the interior of the tube was entirely lined with its crystals. The centre, which was at first limpid, became gradually yellowish, and afterwards brownish nearer the crystallized ammoniacal salt; this arose from the spontaneous decomposition which hydrocyanate of ammonia suffers. This salt did not contain any water chemically combined; for it was crystallized in the form of little rectangular tables and prisms, which are the usual forms of this salt when it is prepared with ammoniacal gas and hydrocyanic acid free from water. The yellowish mass contained in the retort was cyanuret of iron, the lower part of which had begun to decompose before the upper part was entirely deprived of water and hydrocyanate of ammonia. By continuing the heat, the cyanuret of iron became of a deeper colour; first, brown; and at last, black, and disengaged azotic gas.

In order to satisfy myself that the decomposition was complete, I placed the small retort in a charcoal fire to make it red-hot, when, to my great surprise, the coaly mass burned vividly, as if oxygen gas had been present in the retort. A small quantity of undecomposed cyanuret of iron disengaged azotic gas with so much rapidity, that a part of the mass was thrown into the neck of the retort. When the retort was cold, it was found to contain a black pulverulent mass, weighing 25.25 per cent.

of the hydrocyanate employed. This substance, when heated in the flame of a lamp, took fire, and continued to burn like ammoniac, but without either smoke or smell; and it left a residue of oxide of iron, the weight of which was precisely equal to that of the mass before combustion.

In another experiment, I heated the hydrocyanate by a spirit lamp, taking care not to heat it enough to occasion the deflagration which occurred in the former case; but the heat was continued as long as the disengagement of gas was perceptible, and the products of the distillation were passed through a tube containing muriate of lime, and the gases received over mercury. The coaly residuum weighed 26.3, and gave by combustion in an open vessel exactly 26.3 of red oxide of iron. It burned as before, but without the deflagration abovementioned occurring at the temperature required to inflame it. The muriate of lime gained 9.7 per cent. of the weight of hydrocyanate. When heated, it gave out at first much hydrocyanate of ammonia, afterwards ammoniacal gas, and lastly water much impregnated with ammonia. In this experiment, the quantity of water could not be determined, but it proves nevertheless that the salt cannot contain more water than is requisite to convert the cyanuret of iron into hydrocyanate: for otherwise the muriate of lime ought to have gained much more. The gases produced contained no trace of carbonic acid. I introduced some lime-water which absorbed a great quantity without becoming turbid. The liquid contained much hydrocyanate of ammonia, and the unabsorbed gas was azote. The explanation of these phenomena is very simple: the hydrocyanate of iron and ammonia is decomposed by heat, and gives hydrocyanate of ammonia, cyanuret of iron, and water. The green colour which appears during the decomposition appears to be owing to the formation of a small quantity of prussian blue, which always begins the decomposition of this salt, even when in solution. When the heat is increased, the cyanuret of iron is decomposed, azote is disengaged, and the carbon remains combined with the iron. As cyanuret of iron contains four atoms of carbon to one of iron, the same proportions ought to be found in the carburet which results from its decomposition. Then in comparing the weight of four atoms of carbon with that of three atoms of oxygen, the first is found to be 301.66, and the last 300; so that when the quadricarburet of iron is converted into red oxide of the metal, its weight should not alter more than 1-1000th, which is too small to be ascertained in experiments made upon so minute a scale as mine. As to the deflagration, I shall hereafter have occasion to mention it.

I tried several times to expose the hydrocyanate of iron and ammonia to temperatures very gradually raised in order to see whether it would not be possible to separate the water, and preserve the hydrocyanate; that is to say, to reduce this salt to the composition supposed in the first case; but I have not

succeeded. The water and the hydrocyanate are always disengaged together, and the salt becomes green. Even when after being highly dried, it gave a residuum by combustion of 28.5 per cent. of oxide of iron, it still yielded water by distillation; so that in drying it, I only commenced its decomposition, which is facilitated by the great tension of hydrocyanate of ammonia, and by the tendency of the hydrocyanate of iron to form prussian blue.

IV. *Prussian Blue.*

I prepared this substance by dropping a solution of cyanuret of potassium and iron into one of muriate of iron, to which I had previously added excess of acid. I washed the precipitate thoroughly and dried it. Prussian blue retains hygrometric moisture so strongly, that sulphuric acid in vacuo does not detach it; and I have no doubt but that this salt when dried would possess the property of causing water to freeze in vacuo quite as well as sulphuric acid and some other substances.

I put some prussian blue into a cylindrical glass which I placed in a small sand-bath. I placed the bulb of the thermometer in the middle of the prussian blue, and heated the sandbath gradually until the thermometer rose to 307° Fahr. The prussian blue did not appear at all changed, and gave out no odour either of hydrocyanic acid, or hydrocyanate of ammonia. I afterwards placed the sand-bath thus heated in vacuo, with sulphuric acid, and suffered it to cool. A portion of prussian blue thus dried was weighed as quickly as possible, and then lighted at the edge. It continued to burn by itself like amadou, giving a vapour which condensed upon a funnel inverted over it: it was carbonate of ammonia. One hundred parts of prussian blue left a residuum of 60.14 parts of red oxide of iron, containing no potash.

It is well known that when a solution of protoxide of iron is precipitated by prussiate of iron and potash, a white insoluble compound is formed which contains potash, and which by absorbing oxygen becomes blue. It is also well known that a salt with base of protoxide, which absorbs oxygen without there being at the same time an increase of acid, combines with an excess of base. Prussian blue, therefore, which is prepared by oxidation of the white precipitate, cannot be a neutral compound.

I added a solution of neutral muriate of deutoxide of iron to a solution of cyanuret of iron and potassium, and I left the mixture exposed to the air until it became blue. The liquid which still contained undecomposed cyanuret, remained neutral; this circumstance proves that no potash was set free, and consequently that the blue compound did not contain a quantity of acid proportional to the capacity of the base which had been increased by oxidation.

Prussian blue thus prepared has properties which it does not

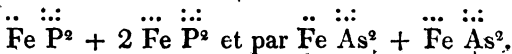
possess when differently prepared, and which may become of some utility in the arts. It is soluble in pure water, but not in water which contains a certain quantity of any neutral salt. It is on this account that it does not dissolve when it is washed, and even the greater part of the saline matter is separated from it. A clear blue solution then passes through the filter; but it again precipitates prussian blue, if it falls into the liquid which had previously passed through.

I made use of a solution of sal ammoniac to separate it from other salts. I washed it with a small quantity of water, until it began to be coloured; and I afterwards dried the precipitate by pressure between paper. The blue solution is precipitated by the addition of muriatic acid, but the precipitate does not lose its solubility in pure water. The blue solution is not rendered turbid by alcohol. This solubility of prussian blue is not always the same. Sometimes the whole of the precipitate rendered blue by oxidation in the air is soluble; at other times, more or less of it remains insoluble. Ebullition produces no change in its solubility. This property in a substance, which under other circumstances is so insoluble, appears to be of the same nature as the solubility of the oxides of tin and titanium, and also of silica, the solubility of which is evidently different in its nature from that of salts. As the addition of excess of acid does not deprive prussian blue of its solubility in pure water, it is evident that this property does not depend upon an excess of base.

In order to compare these phenomena with those which occur in insoluble salts of protoxide of iron, I examined the changes produced by oxidation in some of the latter. A great number of them may be preserved without undergoing alteration, or they become yellow by the formation of a salt with peroxide in excess. But there are two, viz. the phosphate and the arseniate of protoxide of iron (the acids of which combine with bases, according to a different law from other acids), which, by absorbing oxygen from the atmosphere, change their white colour into a darker one, and form salts with excess of base differing as much from salts of the protoxide as of the deutoxide. The phosphate becomes blue; it is even found in nature partly blue and partly white; but in this latter case, it becomes blue in the air in a few days. The arseniate, on the contrary, becomes of a deep green colour. The two varieties of the arseniate appear also to be found in nature. The neutral salt has been lately found in Saxony: it is called *scorodite*; it is a salt which contains water, and has the same colour as protosulphate of iron: the other has been long known; it is the cubic arseniate of iron.

There is, however, an essential difference between these salts and prussian blue. They do not form double salts with other bases; and when they are decomposed by caustic potash, they do not yield hydrate of deutoxide of iron, like prussian blue. On

the contrary, the potash combining with their acids, leaves a substance as black as charcoal, which contains no combined water, and which suffers much less change during washing than the protoxide: it is this compound of deutoxide with protoxide, which I have named *oxidum ferroso-ferricum*; and it is to be presumed that the composition of these salts may be expressed by:



It appeared to me extremely probable that prussian blue might be a completely analogous compound. Its formation by means of double decomposition ought then to destroy the neutrality of the two salts formed, and free acid ought to be liberated.

I prepared a solution of neutral muriate of deutoxide of iron, and after having determined by a correct analysis the relation of the acid to the oxide of iron, in order to satisfy myself of its being neutral, I added it drop by drop to a solution of cyanuret of iron and potassium, which did not change the colour of tournsol. When the greater part of the cyanuret was decomposed, I suffered the precipitate to remain. The clear liquid was found to be as neutral as at first, so that the precipitate was as neutral a compound as the substances employed to prepare it. I continued to add the muriate of deutoxide of iron until it was in excess. The liquid had then acquired the property of slightly reddening the tincture of turnsol, as must happen from the excess of muriate employed. The prussian blue produced in this experiment can, therefore, only be a double hydrocyanate in which the oxygen of the deutoxide is double that of the protoxide.

I afterwards analyzed some prussian blue thus prepared, by digesting it with excess of caustic potash. The undissolved oxide of iron was separated and washed,* and the alkaline liquid (which with the protoxide of the decomposed blue had formed cyanuret of iron and potassium) was decomposed by the addition of corrosive sublimate, the digestion being continued for some hours. The oxide of iron precipitated by this process was washed and strongly heated to separate the oxide of mercury, which was precipitated with it. The oxide of iron, separated from the prussian blue by potash, was to that separated by corrosive sublimate as 30 to 22.

If the prussian blue were analogous to the phosphate and arseniate alluded to, the two weights of the oxide of iron ought to have been in the proportion of 2 to 1. I, therefore, repeated the experiment with the prussian blue, washed, but not dried, in order that it might not be changed by the drying, and the two weights thus obtained were 70.5 parts of oxide of iron, separated by the potash, and 52 parts precipitated by digestion with the

* I satisfied myself by an experiment previously made, that the oxide of iron thus obtained contained no hydrocyanic acid.

corrosive sublimate. These proportions being neither as 1 to 2, nor as 2 to 3, I thought it requisite to examine whether the caustic potash did not in some way decompose cyanogen. I, therefore, analyzed prussian blue by digesting it for a long time with bicarbonate of potash. The decomposition was complete, the bicarbonate left 31 parts of oxide of iron. The yellowish solution was supersaturated with nitric acid, evaporated to dryness, and the dried mass was exposed to a red heat.

This mass being washed with water left 23 parts of oxide of iron undissolved. All these numbers agree together, and the two portions of oxide of iron obtained in each experiment are in the proportion of three atoms of oxide of iron and four atoms of the same oxide; that is to say, = 2935.29 : 3913.72. The analysis then proves decidedly, that in prussian blue prepared in this manner, the oxide of iron contains twice as much oxygen as the protoxide, and consequently its composition is proportional to that of other cyanurets or ferruginous hydrocyanates; this is also demonstrated by the substances from which prussian blue is precipitated, remaining neutral. But what is then the blue mass which is produced by the oxidation of the white hydrocyanate of iron? This compound cannot be neutral, for it does not meet with any acid to saturate it, nor can it contain oxide of iron mechanically mixed; first, because I have often seen it totally dissolve in water; and secondly, because the hydrate of oxide of iron must alter the fine blue colour to green, as happens if the solution in which it is formed contains an excess of the ferruginous salt.

The experiments which I have made to analyze prussian blue by means of combustion have not given me as decisive results as I expected. A quantity of prussian blue dried at a high temperature gave 58 per cent. of oxide of iron by combustion. Another portion of the same prussian blue, weighed an instant after the former, was burned with oxide of copper, taking the same precautions as already mentioned. The water, carbonic acid, and azotic gases, obtained, indicated 45.6 parts of carbon, and 53.1 of azote = 98.7 parts of cyanogen. The prussian blue employed contained such a portion of metallic iron as would combine almost precisely with this quantity of cyanogen to become cyanuret; and by adding together the weight of the iron, cyanogen, and of the water obtained, the weight amounted almost precisely to that of the prussian blue employed, without leaving any thing for the weight of the oxygen of the oxide of iron, which ought to exceed the hydrogen of the hydrocyanic acid, if the compound had contained excess of base.

I repeated this experiment with prussian blue precipitated from a neutral solution of muriate of deutoxide of iron, and dried in vacuo, without heat. It left after combustion 54.66 per cent. of red oxide of iron; when burned with oxide of copper it

yielded carbonic acid and azote which indicated almost precisely the same quantity of cyanogen as in the former experiment. By adding together the weight of the water, iron, and cyanogen, almost precisely equalled that of the prussian blue employed. No one of these experiments indicated such a relation of the iron to the cyanogen as ought to have been obtained according to the experiments made in the humid way, and already described; they leave, therefore, some uncertainty as to the true nature of this substance.

I treated some prussian blue recently prepared, with muriatic acid to separate the excess of base, and after having well washed it, I mixed it with pure water, and passed a current of sulphuretted hydrogen gas through it, until the water was saturated with the gas. I then corked the bottle, and left these two substances to react upon each other for several days. The prussian blue became of a lighter blue colour, and eventually became of a dirty white.

The fluid became whitish with the sulphur precipitated; afterwards separated the white mass, and I evaporated the excess of sulphuretted hydrogen gas by exposure to the air. The fluid now reddened tournsol, and precipitated of a blue colour three salts of iron which contained deutoxide. The sulphuretted hydrogen gas on reducing the deutoxide of iron of the prussian blue to the state of protoxide, and then separated from it that portion of hydrocyanic acid by which the deutoxide had been neutralized in the same way as occurs with every other neutral salt, whose base is a deutoxide, when the sulphuretted hydrogen reduces it to the state of protoxide. It must also be observed that the acid liquid in question contains no pure hydrocyanic acid; it still contained iron: it was the ferruginous prussic acid, which I shall hereafter consider.

The mass which was rendered white by the action of the sulphuretted hydrogen regained its blue colour by exposure to the air, and became at the same time partly soluble in pure water. The blue solution again treated with sulphuretted hydrogen deposited a black substance, did not again become acid, nor did it again acquire the property of precipitating the solutions of deutoxide of iron of a blue colour. In the same manner, the insoluble portion of the regenerated blue became black by sulphuretted hydrogen. These experiments prove then that there are actually two blue compounds, one of which is neutral, and composed of three atoms of hydrocyanate of protoxide and four atoms of deutoxide of iron; that is to say, the last contains twice as much oxygen and acid as the first. The other appears to be composed of an atom of hydrocyanate of protoxide and of two atoms of subhydrocyanate of deutoxide of iron, analogous to the blue phosphate of iron and the green arseniate of the same metal.

It is evident that the phenomena produced by the metallic cyanurets and hydrocyanates can be explained only by a theory analogous to that proposed for the muriates by MM. Gay-Lussac and Thénard, and which was afterwards adopted and developed by Sir H. Davy; and this analogy of the phenomena presented by the cyanurets is undoubtedly a circumstance which is very favourable to this theory.

It seems to follow from the experiments above-mentioned, that the cyanurets of very electro-positive bases; that is, of the metals which form alkalis, do not decompose water, and do not form hydrocyanates. The weaker bases, such as glucina,* ammonia, the greater number of metallic oxides, on the contrary, become hydrocyanates, which, when exposed to a high temperature, either do not become cyanurets, or do not form them without having a part of their cyanogen decomposed by the oxygen of the bases, and yielding at the same time carbonic acid, ammonia, and metallic carburets. With the exception of hydrocyanate of iron and ammonia, it appears that when one of the bases occurs in the state of hydrocyanate, the other is so likewise; so that there is no compound of a cyanuret and a hydrocyanate. When the cyanurets combine with an additional quantity of base, it appears that the cyanuret becomes an hydrocyanate, and that the whole becomes a subhydrocyanate; such is probably the state of combination of cyanuret of mercury with oxide of mercury.

Before I conclude these observations, I shall say a few words upon the acid called ferruginous hydrocyanic acid, and which has been considered as a peculiar acid, of which the iron is an element. This compound is produced when a stronger acid combines with the second base of a ferruginous hydrocyanate; then the whole quantity of hydrocyanic acid combines with the protoxide of iron, and these results are hydrocyanate with excess of acid, in which the protoxide is combined with three times as much acid as in the neutral salt. All that I have already stated upon the nature of the cyanurets and ferruginous hydrocyanates is evidence in favour of this manner of regarding this acid substance. Porrett has described two methods of obtaining this acid, one of which is to decompose the cyanuret of iron and barium by sulphuric acid; and the other to decompose the cyanuret of iron and potassium by means of a solution of tartaric acid in alcohol. The fluid being left to spontaneous evaporation, crystals are obtained. Neither of these methods gives the superhydrocyanate of iron in a pure state.

* Glucina gives a soluble hydrocyanate, which, reduced by evaporation, becomes a transparent varnish, which is often bluish. It is obtained by the double decomposition of sulphate of glucina and cyanuret of lead and iron. There is also a ferruginous hydrocyanate of alumina. When neutral, it is but slightly soluble; but with excess of acid, it may be dissolved. It is prepared with hydrate of alumina and ferruginous prussic acid.

I prepared it in the following manner: I took some cyanuret of iron and lead which had been well washed but not dried, and I decomposed it under water by a current of sulphuretted hydrogen gas, until the sulphuretted hydrogen was in excess. I immediately filtered it, and evaporated it in vacuo in the usual manner. But as sulphuretted hydrogen spoils the air-pump, I afterwards took the precaution to decompose the sulphuretted hydrogen by the addition of a small quantity of cyanuret of iron and lead. The filtered fluid remained limpid and colourless in vacuo, and it eventually leaves a milk-white opaque substance, which has no appearance of crystallization. This white matter has the following properties: it dissolves in water, to which it imparts an acid and agreeable flavour, but which is rather astringent. In contact with the air it deposits prussian blue, and assumes a greenish colour. It is inodorous, unless it has begun to decompose. When boiled, the liquid gives out hydrocyanic acid, and deposits a powder which becomes blue in contact with the air. It is necessary to boil it for some time to decompose it entirely. The fluid, when half decomposed by ebullition, has seemed to me to possess a more astringent taste; I will not assert, whether by this operation, there is formed an hydrocyanate of iron with a smaller excess of acid. If cold water be saturated with dry superhydrocyanate, and the solution be suffered to remain, it gives small transparent colourless crystals which appear to contain water of crystallization, but I have not been able to determine their form. The crystals are formed in groupes composed of concentric rays. The rays appeared to me to be quadrilateral prisms. If I were permitted to conjecture, I would say that these crystals are a hydrocyanate in which water replaces the second base that existed with the protoxide of iron. The white substance obtained by evaporation in vacuo does not appear to contain any water, or rather appears to be the superhydrocyanate of protoxide of iron without water of crystallization; for if it be distilled in a small and proper apparatus, it gives at first hydrocyanic acid; afterwards carbonate of ammonia and prussiate of ammonia. The production of ammonia in this experiment proves that what remains after the hydrocyanic acid which is first evolved is a hydrocyanate, and not a cyanuret, because, in the latter case, it could only have given hydrocyanic acid and azotic gas. This substance may be kept without alteration in well-closed vessels, but in the air it gradually decomposes, becomes at first greenish, afterwards blue, and finishes by being entirely converted into prussian blue.

V. On the Decomposition of Hydrocyanates by Exposure to a High Temperature in close Vessels.

It is evident from what I have above stated that several cyanurets when exposed to heat in closed vessels must exhibit

phenomena, differing from those which have been hitherto admitted.

I have examined some of them with this view, and I think that I can draw conclusions from my experiments regarding the whole class of cyanurets.

1. *Cyanuret of Iron and Potassium*.—I heated this in a small apparatus, so arranged as to collect the gases evolved. At a heat near redness, it melts; and before this, it gives nothing volatile or gaseous. But at a strong red heat, it is filled with small bubbles which are disengaged at long intervals, and it remains in this state, even at a temperature at which the glass softens. While cooling, it has a deep-yellow colour; but it becomes almost colourless upon cooling to the temperature of the atmosphere. Dissolved in water, it leaves a small quantity of quadricarburet of iron in black flakes, and the solution has alkaline properties and smells of hydrocyanic acid. The gas obtained possessed all the characters of azote. The cyanuret of potash separated from cyanuret of iron thus decomposed gives hydrocyanate of potash with water, from whence its alkaline property and hydrocyanic odour are derived.

2. *Cyanuret of Iron and Barium* treated in the same manner is more easily decomposed at a red heat, gives abundance of azotic gas, and leaves a residuum of cyanuret of barium and quadricarburet of iron. The decomposition is usually so complete, that the solution gives no blue with solutions of deutoxide of iron; but there are obtained a solution of a fine purple-red colour, and a red precipitate. This red compound was first observed by M. Vauquelin: he obtained it by treating deutoxide of iron by hydrocyanic acid. That which was obtained in the manner now described was not decomposed by ammonia; and after being evaporated to dryness, it was again partly soluble in water. Another part appeared to be decomposed, and was changed into a greenish mass.

3. *Cyanuret of Iron and Calcium* is still more easily decomposed than that of barium. As it retains more water relatively to its volume, it gives small drops of water, and a little carbonate and hydrocyanate of ammonia. Towards the end, and when the heat becomes red, it burns, but not vividly, in the same manner as before observed, with respect to the hydrocyanate of iron and ammonia.

4. *Cyanuret of Iron and Lead*.—If it contains water, hydrocyanate of ammonia is obtained, which readily decomposes, and becomes brown. At a red heat, it begins to evolve azotic gas, and gives nothing else. When the disengagement of gas is finished, if the retort be put in the middle of the fire, vivid combustion is produced. If this temperature be employed before the cyanuret is decomposed, the disengagement of azotic gas occurs with such rapidity during the combustion, that a portion of the coaly mass is carried up with it. If all the water has

been separated from the cyanuret before the commencement of the experiment, the residual mass is a double carburet, composed of an atom of quadricarburet of iron and two atoms of quadricarburet of lead, $\text{Fe C}^4 + 2 \text{Pb C}^4$. If, on the other hand, the cyanuret contains water, one part of the carbon of the cyanogen is converted into carbonic acid at the expense of the water, and there is a deficiency of carbon in the quadricarburet of iron. It has been supposed that the residual mass, after the decomposition of cyanuret of iron and lead, is a kind of pyrophorus; this is an error probably derived from the circumstance of the carburets thus obtained taking fire readily at a lower temperature than is sufficient to inflame other bodies, so that, if the retort be broken before the contents are cold, they take fire, and continue to burn like amadou.

5. *Prussian Blue*.—Prussian blue treated with muriatic acid was dried at the temperature of 336 Fahr. and afterwards decomposed. It gave at first pure water, then a little hydrocyanate of ammonia, and afterwards a great quantity of carbonate of ammonia, but always followed by moisture. After the evolution of the volatile bodies, I placed the retort in the middle of burning charcoal, and there was produced a quick and brilliant combustion, as with the hydrocyanate of iron and ammonia.

Fifty parts, and 7-10ths of the black mass which remained in the retort were burned in a small capsule previously weighed, 54.86 parts of red oxide of iron were left. This relation between the weights of the carburet of iron and that of the oxide, agrees precisely with a tricarburet of iron Fe C^3 ; for, according to calculation, 50.7 of tricarburet of iron ought to give 54.89 parts of red oxide of the metal. As water accompanies the products of the decomposition of prussian blue, from the beginning to the end, it is evident that the affinity of iron for carbon keeps them in a fixed state of combination, and consequently the proportion of carbon in the carburet of iron obtained by the decomposition of prussian blue is not an accidental circumstance.

6. *Hydrocyanate of Iron and Copper*.—This compound contains water, besides that which converts it into hydrocyanate. It gives during decomposition much water, some carbonate, and hydrocyanate of ammonia. The phenomena of combustion may be produced in the mass, but for this purpose, a very high temperature is necessary, and yet it is not very brilliant. The residual mass is black, and possesses the external characters of those already described. It inflames readily, and continues to burn by itself; 27.7 parts of carburet yielded 28.9 of the two oxides, both at the maximum of oxidation. The numbers agree with a compound of an atom of quadricarburet of iron and two atoms of bicarburet of copper; that is to say, $\text{Fe C}^4 + 2 \text{Cu C}^2$.

7. *Hydrocyanate of Iron and Cobalt*.—If this hydrocyanate is well dried, it gives only a small quantity of water, carbonate and hydrocyanate of ammonia, at the boiling point of mercury. Its

deep green colour changing at the same time, and becoming of a lighter green. At a higher temperature it blackens, gives azotic gas, and finishes by deflagrating. The black mass appears to be $\text{Fe C}^4 + 2 \text{Co C}^4$, of which a small quantity of carbon is accidentally lost by the presence of an indeterminate quantity of adhering moisture.

8. *Cyanuret of Mercury* has been so thoroughly examined by M. Gay-Lussac that I have nothing to add, excepting that the coaly mass which remains after the decomposition of this cyanuret is derived from the formation of carburet of mercury during the decomposition, and this is the reason why the cyanogen is always mixed with azotic gas. It is this carburet which renders the mass black, and at last, when exposed to a high temperature, the mercury is volatilized, and the charcoal remains.

There is also a double cyanuret of iron and mercury, or rather a double hydrocyanate of protoxide of iron and deutoxide of mercury. It is obtained by dropping a solution of corrosive sublimate into one of cyanuret of iron and potassium. A white precipitate is formed; but it is decomposed not only by ebullition, which causes the cyanuret of mercury to dissolve, and leaves that of iron insoluble, but also by the contact of the air, which causes the cyanuret of iron to become prussian blue; so that I have not been able to obtain it in a dry state.

9. *Cyanuret of Iron and Silver* becomes bluish by exposure to a little too much during drying. It is a cyanuret, and not an hydrocyanate. When decomposed, it gives at first cyanogen, and afterwards, when the cyanuret of iron begins to decompose, it gives azotic gas. The phenomenon of combustion occurs at a lower temperature than with other cyanurets. The residual mass is a mixture of metallic silver and quadricarburet of iron, from which the silver may be separated by being well triturated with mercury.

(To be continued.)

ARTICLE VIII.

On the Application of Howard's Fulminating Mercury to the Discharge of Ordnance. By John Deuchar, MWS. Lecturer on Chemistry in Edinburgh.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Edinburgh, March 7, 1821.

I HAVE just read a communication from your correspondent T. N. R. M. in the number of the *Annals* for this month. He seems to have overlooked that part of my paper near the top of page 91, in which I mention that I had used fulminating mercury, *New Series*, VOL. 1.

and that it rent asunder the steel plate at the top of the apparatus without firing the gunpowder at the bottom. However, as he suggested the trial of different proportions of the fulminating mercury with sulphur and charcoal, and that too with the knowledge of mixtures of these three ingredients having been used in several secret experiments some time ago performed in Paris, I have repeated my experiments with Howard's fulminating mercury weakened with the above two inflammables. I first tried to fire gunpowder with it, through flannel, at the bottom of the apparatus (Plate III. fig. 1), p. 89, when I found that it not only did not inflame the gunpowder, but failed completely in tearing, or even moving, the paper at the bottom of the tube. I next tried the effect of exploding the mixture at the top, when the tube presented no resistance at all to any flame that might pass along, but I found that no light appeared at the bottom, although I performed the whole in a dark situation. I then shortened the tube, but still found that there was no flame sent out at the end of it.

In some of these experiments, equal parts of Howard's fulminating mercury, sulphur, and charcoal, were used; in others, two parts of the mercury were taken to one part of each of the rest; and in others again the proportions were still further varied.

I remain, yours respectfully,

JOHN DEUCHAR.

ARTICLE IX.

Notice of a Paper laid before the Geological Society on the Structure of the Alps and adjoining Parts of the Continent, and their Relation to the Secondary and Transition Rocks of England.
By the Rev. W. Buckland, Professor of Mineralogy and Geology in the University of Oxford, FRS. FLS. MGS. &c.

THE detail of the phenomena of which I have endeavoured to include a brief summary in this prospective notice, will form the subject of a future and more extensive communication to the Geological Society. My immediate object is to present an abstract of the leading points of resemblance between the rocks of the Alps, and those which occur in our own country.

Of the primitive alpine rocks that form the central axis of this most elevated and most important mountain chain in Europe, I have only to observe that they present such an identity of substance and circumstances with the primitive rocks of other parts of the world, that any detailed account of them will be unnecessary.

But with regard to the transition and secondary strata that

occur in these same elevated regions, much difficulty exists, and much error prevails, which it will be my object to endeavour to do away.

The term transition (for example) is applied by many of the first geologists of the Continent to a class of alpine rocks of the same age with those which in England are justly considered secondary, and which constitute the new red sandstone, or red rock marl formation, of the English geologists; and the alpine limestones which have been supposed to have pretensions to high antiquity will be found on closer examination to be contemporaneous with the magnesian limestone, and oolite formations of England, and consequently more recent than our great coal formation and mountain limestone; with which latter its name, external aspect, and elevated position, would at first seem to associate it. It will be found moreover that the mountain limestone and great coal formation of England do not occur in the Alps.

The tertiary formations also constituting the molasse and nagelfluë of the Great Valley of Switzerland have been mistaken for the new red sandstone beds of England.

The causes of these mistakes I shall point out, and endeavour to do away; they consist partly in the enlarged bulk, and partly in the want of distinct features, and of tangible character, which accompany all secondary strata as they enter the Alps.

I shall hope, however, to prove their identity with English formations by the evidence of actual sections; and to show that a constant and regular order of succession prevails in the alpine and transalpine districts, and generally over the Continent, and that this order is the same that exists in our own country. But though referrible to the same system, and coeval in point of time, and conformable with respect to their relative order of succession, the formations of England and the Alps are much disguised by local circumstances, and present widely varying features, the extremes of which it would be impossible to identify without the fortunate interposition of certain connecting links that are equally related to, and partake equally of, the characters of them both.

The most remarkable anomaly is the total absence of the English mountain lime and coal formations; while our oolite and magnesian limestones (under the name of alpine limestones) rise unto the most elevated crests and pinnacles that crown the summits of this gigantic chain. The following are among the greatest heights which they occasionally attain:

	Fath.
Ortler, in Tyrol	14,466
Jungfrau, in Switzerland	12,872
Dödi Berg, in ditto	10,059
Tiltis	10,000
Diableretz	8,240
Dent de Morcle	7,600

In the Pyrenees also we find the same limestones forming the most elevated ridge and great water shed of that vast chain, and rising in Mount Perdu to 10578 feet, and in the Torre de Marbore to 10,260.

These alpine limestones include nearly all the calcareous formations of England, from the magnesian limestone which lies next above our coal measures upwards to chalk, piled on each other without any subdividing strata of clay or sand, and all assuming the common character of a compact grey marble, possessing no variations by which one part of the formation can be distinguished from another. And such is generally the featureless condition of the great calcareous masses, which, extending from the Pyrenees through the south of France by Avignon to Dauphiné, stretch thence uninterruptedly through Switzerland, Tyrol, Salzburg, and Styria, to the Danube below Vienna; while on the south side of the central Alps a similar calcareous mass extends from the Lago Maggiore and Como through the Italian Tyrol into Croatia and Dalmatia.

Fortunately this want of distinguishing feature is not universal; occasionally spots occur in which the strata present evidences which identify them with those of England, and throw light on the history of the larger and less distinctly characterized masses of which they form a part.

The general structure of the Alps and Jura mountains, and the valleys adjacent, reduced to their most simple form, may be briefly stated thus :

1. *General Structure of the great Alpine Chain.*

The central axis of this vast chain extending continuously north eastward from Savoy through Switzerland, Tyrol, and Styria, to Presburg, is composed of primitive rocks, the average breadth of which is about 60 miles. In contact with these are extensive masses of transition rocks, but their presence is irregular; and when they occur, there is nothing in the external features of the country to mark their junction with the primitive masses.

This central ridge of primitive and transition rocks is bounded on each side by two vast belts of alpine limestone, coextensive with, and continuous beyond the primitive chain. The elevation of the latter sinks gradually as it advances north eastward, till, near Presburg, it drops below the bed of the Danube; and on the north of that river again gradually rises to form the chain of the Weisse Gebirge which connects the Alps with the Carpathians.

The lateral belts of alpine limestone maintain their elevation more constantly than the primitive chain, and extend themselves far beyond it; the north belt stretching north-westwards from Dauphiné into Languedoc, and to the Pyrenees, and the south belt south-eastward by Carniola into Dalmatia.

These lateral belts are divisible into two systems, the elder and younger alpine limestones; the former contemporaneous

with the magnesian limestone that rests in the coal formation of England, and characterised by containing gypsum, salt and metallic ores, and occasionally beds of saliferous marl, red sandstone, and rouhwacke; the latter entirely destitute of all these substances, and comprehending beds which their organic remains and superposition to the elder alpine limestone and occasional structure show to be contemporaneous with the lias and oolite, and sometimes green sand and chalk formations of England. The chalk seems to occur only on the Italian side of the Alps, near Vicenza.

These beds of alpine limestone are bounded externally by the Tertiary formations of the Plain of Lombardy on the south; and by similar tertiary formations in the Great Valleys of the Danube, and of Geneva on the north.

Internally, they are terminated by two precipitous escarpments; one on each side of, and both rising towards, the central primitive ridge.

Between these two escarpments there are detached ridges and insulated or outlying masses of the same alpine limestone covering occasionally large tracts of the intermediate primitive country.

Note.—The great north escarpment produces a remarkable effect upon the upper courses of all the important rivers that rise on the north side of the central watershed of the Alps, i. e. the Iserre, Rhone, Rhine, Inn, Salza, and Enns.

Most of their tributary streams take origin in the central primitive ridge, and descend northwards till they meet the great escarpment of the alpine limestones. On approaching this, they are suddenly deflected nearly at right angles to their former direction, and run under it from 50 to 100 miles, till some gorge or fracture in the limestone allows them to escape into the plains.

The above rivers enter their gorges at the following places: The Iserre at Grenoble. Rhone at St. Maurice. Rhine at Sargans. Inn above Kufstein. Salza at Werfen. Enns at Hiflau, 10 miles north of Eisenertz, in Styria.

The extent of their course under the escarpment is as follows:

- Iserre from Conflans to Grenoble.
- Rhone from the Furka to St. Maurice.
- Vorder Rhine from its source to Coire.
- Inn from Landek to Innspruck and Kufstein.

The course of the Upper Salza is not in the valley immediately subjacent to the limestone escarpment, but in a parallel valley of primitive rocks at no great distance to the south of it.

Some minor rivers between this valley and the escarpment are similarly affected by it, and escape by minor gorges.

Enns from Radstadt to Hiflau.

The south calcareous belt appears to dip (at least at its west extremity near Como and Verona) towards the plain of Lom-

bardy, and sink beneath the tertiary formations of the basin of the Po; while the north calcareous belt, being disposed in a kind of trough shape, has a double dip and double escarpment, dipping northwards along its line of junction with the central primitive chain, but changing its inclination, and again rising with an highly elevated escarpment also towards the north, along the line of its termination in the Valley of the Danube and of Switzerland.

At the foot of this north escarpment, the limestone rests generally on a basis of greywacke, dipping southwards under the limestone towards the central Alps, and being often much distorted and even vertical.

The same greywacke appears irregularly along the base of its south escarpment, and may be seen at Werfen, 50 miles on the south of Salzburg.

The south calcareous border rests sometimes immediately on primitive slate without the intervention of any transition rocks, but more frequently upon an extensive deposit of red sandstone, red conglomerate, and porphyry, which are considered as belonging to the transition formation by Brocchi and most writers on the Alps, but which appear to be contemporaneous with the new red sandstone formation of England.

The following order of succession, founded principally on the rocks that occur in Switzerland and the Tyrol, may be considered as a type of the general structure of the Alps and valleys adjacent to them. I shall arrange them in classes according to the names by which they are generally known on the Continent, and point out the analogies which the component members of each class bear to the strata of England.

1. *Tertiary Formations.*

These formations are principally composed of strata of sand, sandstone, clay, and limestone, of nearly the same age with the formations above the chalk in the basins of London and Paris, and containing subordinately beds of marlstone and coal, in both of which are occasionally found freshwater shells. The sandstone and conglomerate constitute the molasse and nagelflue of Switzerland, and their order of succession is; 1. or uppermost, nagelflue; 2. freshwater formations; 3. molasse. The thickness of 1 and 3 is from 3000 to 4000 feet. The occurrence of freshwater formations is partial, and their thickness not considerable.

The molasse and nagelflue occupy the Great Swiss Valley extending from the Lake of Geneva to that of Constance, from whence they continue down the Valley of the Danube into the plains of Hungary and Transylvania: they are occasionally accompanied by limestone that bears a strong resemblance to the calcaire grossier of Paris.

II. *Younger Alpine Limestone.*

1. *Chalk, assuming the form of hard compact limestone,*

passing, as in Lincolnshire, from brick-red to pure-white colour, and containing nodules of black flint, occurs in the Subalpine Hills of the Vicentino on the north of Vicenza and Verona, and at Monteselice near Padua.

2. *Green Sand*.—Dark sandy limestone, interspersed with grains of green earth, and containing abundantly the same organic remains with the green sand formation of England, especially that near Folkstone. It occupies a considerable extent in Savoy and Switzerland, where it rests on oolite, and constitutes the most recent beds of the younger alpine limestone, forming the summits of the Varens, Buet, Dent de Morole, and Diableretz mountains, at an elevation of 7000 or 8000 feet, and ranging in a line parallel to the central primitive chain of Mont Blanc across Savoy from the valley of the Arve to that of the Rhone.

3. *Oolite, or Jura Limestone*.—The two principal varieties of this formation are: 1. A compact grey marble; 2. A granular oolite; the latter occurs abundantly in the Tyrol, in the valley of the Adige below Trent, and occasionally in the Salzburg mountains; the former prevails in Switzerland, and generally through the Alps; near Aigle, on the south-east of Vevey, it assumes the character of red compact marble similar to that of Salzburg; and at Roche, in the same neighbourhood, it is full of organic remains resembling those of the English coral rag; but from the compact nature of the matrix in which they are imbedded, these are visible only on the surface of the weathered blocks. This last observation may be applied also to a large proportion of the younger alpine limestone beds in the Tyrol and Salzburg, in which the organic remains are for the most part concealed by the extreme compactness of the stone; but, when apparent, are referrible to the same classes with the oolite fossils of England. Such remains are distinctly visible at Nafels, near Glarus, in Switzerland, and at Halstad, in Salzburg; where also the limestone becomes partially oolitic.

4. *Lias*.—The lias (like all the other formations in the Alps) is destitute of its alternating beds of clay, but maintains its position between the oolite and new red sandstone. At the salt mines of Bex, it reposes immediately on the upper bed of saliferous gypsum, where it is a dark-blue compact limestone, and contains ammonites, nautilites, terebratulites, and many bivalves identical with those of the lias of England.

At Halstad, it occupies a similar position between the oolite and red marly sandstone that covers the saliferous limestone, and is full of ammonites, belemnites, and other lias fossils. At Seefeld, near Inspruck, it contains fish similar to those which occur in the English lias at Lyme Regis. And at Mischelle, near Trent, it may be seen dividing the oolite from the red marl and new red sandstone.

III. Elder Alpine Limestone.

Magnesian Limestone of England.—The thickness of this formation in the Alps is very considerable, and we must refer to it a large class of rocks which are placed by Brocchi, Charpentier, and other writers, among transition limestones. Mr. Ebel has lately proposed to call them by the name of Hochgebirgs Kalkstein (high mountain limestone) from the great elevation they usually attain. The limestone is here and there precisely of the same character with the English magnesian limestone; but in general it is less abundantly charged with that earth than it is with us, though it presents at intervals nearly all the varieties that attend this formation in England and Germany. Its prevailing character in the Alps is a dark compact limestone, not easily distinguishable from the compact younger alpine limestone without the aid of its occasional metallic contents, and of the subordinate beds that occur in it: the latter are referrible to the following strata of the magnesian limestone formation that are well-known in Germany.

1. *Zechstein*.—Dark compact stratified limestone sometimes much charged with silex, alumine, or bitumen. Localities: Axenburg, on Lake Lucern, north border of Lake Wallenstadt, Ischell salt mine in Salzburg.

2. *Asche*.—Dark grey limestone, of minute sandy grain, rough to the touch, and decomposing to loose powder, resembling ashes. Its most compact varieties are often split into small angular fragments, which are reunited into an irregular breccia.

Localities: Pass of Guncles under Galanda Berg on the west of Coire, in Switzerland. Reichenhall salt springs in Salzburg.

3. *Rouhwacke*.—A brecciated limestone composed of an agglutination of angular fragments of dolomite, not rolled, but formed apparently during the consolidation of the compound rock in which they are inclosed. The fragments are more loaded with magnesia than the matrix in which they lie; they vary in colour from yellow to red, have a pearly glimmering aspect, and decompose spontaneously to a yellow powder, leaving small angular cells; the intercellular substance forms a hollow rugged mass, to which the term *rouhwacke* (roughstone) has been applied in Germany.

This variety is usually found near the deposits of salt and gypsum, and also in the beds of asche and zechstein.

Localities: Bex Salt Mine. Leisigen on Lake Thun. Weiss-tannen, near Lake Wallenstadt. Mischell, near Trent, on the Adige. All the Rock Salt Mines of Tyrol and Salzburg.

4. *Rogenstein* (Roestone).—An oolitic form of magnesian limestone, well-known in the Thuringervald, and which has often been mistaken for the English oolite, or Jura limestone, but differs from it in being of much higher antiquity as well as

in character and composition. The Jura and English oolite being usually of a light-yellow colour, inclining to white; while the rogenstein is of a dark-brown colour, passing to dull-red: its oolitic concretions are of concentric structure, and vary in size from minute grains to that of a large bean. In England they attain the magnitude of a cannon-ball at Sunderland near Newcastle, and are of a lighter colour than in Germany.

Localities: Hall Salt Mines, near Inspruck; near the bridge of St. Maurice, in Switzerland; Bridge, three miles south of Reichenhall, in Salzburg, on the road to Bertolsgarten.

All these varieties of the magnesian limestone formation, or elder alpine limestone, lie in the midst of, and often pass into, a dark compact grey limestone. In the same limestone there occur also subordinate masses of dark blue shale, and of red sandstone; these may be seen in all the salt mines of Tyrol and Salzburg, where they have masses and veins of salt and gypsum dispersed throughout their substance. The gypsum is usually attended with much anhydrite, sometimes forming (as at Bex) the larger portion of the entire mass. The gypsum is often destitute of salt, but the salt rarely (if ever) occurs unattended by gypsum. Deposits of gypsum occur abundantly in other strata; but salt seems to be confined exclusively to the component rocks of the magnesian limestone and new red sandstone formations.

Localities of salt: Hall, Halstadt, Halheim, Aufsee, Bex.

Salt is known to occur in Switzerland only at Bex; but the gypsum extends from thence north eastwards in a line parallel to the central chain to Leisegen on Lake Thun by the valleys of Gsteig, Ander Luc, Lauenen, and Adelboden. It is found also on the south-west of Bex at Val de Lie and other places on the continuation of the same line through Savoy, south westward.

IV. *Transition Rocks (improperly so called.)*

Alternating with the lower members of the magnesian limestone, and interposed between them and the true transition rocks is an important deposit which must be separated from the latter class, and placed with the new red sandstone, and new red sandstone conglomerate of England, although ranged among transition rocks by most writers on the Alps, and by Brocchi in his Account of the Val di Fassa, in Tyrol.

This alpine formation admits of subdivision into three varieties: 1. New red sandstone; 2. New red conglomerate of England, or old red sandstone of Werner; 3. Porphyry.

1. *New Red Sandstone, or Bunter Sandstone of Werner.*—The component beds of this formation are seen to great advantage in the Valley of the Adige, 10 miles north of Trent, and in the Valley of the Avisio, which descends from the Val di Fassa into that of the Adige in the same neighbourhood. In both these valleys they occupy the lower regions of mountains, whose sum-

mits are composed of alpine limestone: their ordinary form is a slaty micaceous sandstone, resembling red greywacke, alternating with beds of marly sandstone and red marl; in the two latter, there occur abundantly beds and nodules of gypsum, and of yellow sandy magnesian limestone. A similar deposit of red marly sandstone may be well seen at Halstad between the saliferous gypsum and lias.

In Switzerland, the usual state of this stratum is more compact, forming a hard micaceous slaty sandstone, resembling red greywacke slate, and variegated with spots of green. It occupies a very considerable extent in this country, running in a line parallel to all the other great formations from Notre Dame D'Abondance, in Savoy, to the Valley of the Rhine, on the east of Glarus, and being visible in the Nieder Simmen Thal on the south-west of Thun; in the Melch Thal, near Lake Sarnen; in the Valley of Unterchachen, near Altorf; at Schwanden, in Glarus; and at Weistannen and Mels, near Sargans, above the head of the Lake of Wallenstadt. Large blocks of it have been drifted down from this last named district over the hills of tertiary formation that border on the Lake of Zurich. It occurs also in the same compact slaty form, accompanied by gypsum, and lying above greywacke, at Werfen, in the Tyrol, 50 miles on the south of Salzburg.

2. *New Red Conglomerate (old Red Sandstone, or Rothe Todte Liegende of Werner).*—As in England and Germany, so also in Switzerland, and the Tyrol: the lower strata of the new red sandstone formation pass into a coarse conglomerate, containing fragments of the adjacent rocks older than itself, united by an argillaceous or siliceous, and sometimes by a semiporphyrific cement. The same characters which this rock presents near Exeter and Teignmouth, in Devonshire, and at the base of the Thuringer-vald, in Germany, are maintained by it in the Alps. It occurs in Italy, near Lugano; in the Tyrol, near Botzen, and Cavalese; and in Switzerland, in the Mount Neisen, near Lake Thun; and at Schwanden, in Glarus. The relative position of this conglomerate, and the new red sandstone, may be seen to advantage in the steep sides and escarpments of the Glarus mountains, between Schwanden and Matt, where they appear beneath well characterized strata of the elder alpine limestone formation, and incumbent on true transition rocks; i. e. the greywacke and greywacke slate of Blattenburg.

3. *New Red Porphyry.*—In Switzerland, the conglomerate of Schwanden seems almost, but not quite, to pass into the state of new red porphyry, its cement and many of its concretions being imperfectly crystalline; but no decided porphyry of this formation occurs on the north frontier of the central Alps. On their south side, however, near Botzen, there is an extensive range of low mountains, composed exclusively of this porphyry: it may be seen reposing on the new red conglomerate by the road side

on the south of Clausen; and covered by the same conglomerate and by new red sandstone, on the south of Botzen, from Neu Markt, in the Valley of the Adige, to Cavalesse, in the Valley of the Avisio.

Along this line the porphyry seems to pass insensibly into the conglomerate, each containing occasionally subordinate masses of the other, and both presenting features which seem to induce us to refer them to a contemporaneous origin; i. e. to consider the porphyry a crystalline form of the same matter which, in its coarser state, constitutes the red earthy cement of the pebbles composing the conglomerate.

This conclusion is borne out by similar phenomena in the same formation in England, Scotland, and Ireland.

V. *Transition Rocks (properly so called).*

Transition rocks, identical in every respect with the greywacke and greywacke slate of England and Germany, occur abundantly in the upper part of the Valley of Glarus, lying beneath, and possessing very different characters from the new red sandstone and conglomerate beds which are usually, but improperly, called transition rocks.

The slate quarries of Blattenburg, well known for their fossil fish, lie in a calcareous variety of this greywacke slate, which alternates with decided granular greywacke. The same transition formation extends from thence to the Valley of the Tamina, above the baths of Pfeffers, and the mountains that flank each side the Rhine for 10 miles north of Coire, where also the slate frequently becomes calcareous.

M. Brochant, in his paper on the Tarentaise and Upper Districts of the Iserre, in Savoy, has described with much accuracy a considerable extent of similar transition rocks on the southwest of Mont Blanc, passing from fine greywacke slate through various kinds of greywacke to coarse conglomerates, some containing calcareous, others siliceous and granitic fragments. Coeval and connected with the latter is the famous conglomerate of the Vallorsine described by De Saussure, being a puddingstone of the highest antiquity, with fragments of gneiss, granite, and clay slate: these transition conglomerates alternate with micaceous slate, serpentine, and quartz rocks; and are totally different in appearance, as well as antiquity, from the new red sandstone conglomerate of Schwanden.

We have analogous formations in this country in the conglomerates which in Pembrokeshire and near Killarney alternate with greywacke and greywacke slate. The old red sandstone of England is nearly connected with these last mentioned conglomerates, being intermediate between them and the mountain limestone, and separated by no strong line from the greywacke formation, to which it is considered as belonging whenever it occurs on the Continent.

Primitive Rocks.

Of the primitive rocks that form the central axis of the Alps, I have only to remark at present, that in consequence of the transition rocks being in many places totally wanting, they often come into immediate contact with the secondary strata.

Trap Rocks.

There are no traces whatever either of trap rocks, or whin dykes, in the Alps of Savoy, Switzerland, or Tyrol, along the entire north side of the great primitive ridge from Mont Blanc to Presburg. But on the south side of it in Tyrol they occur in considerable force at the Val di Fassa on the east of Botzen under circumstances of singular resemblance to the trap rocks and whin dykes of Scotland, and the north of Ireland.

An excellent mineralogical account of this district has been given by Prof. Brocchi, of Milan. The trap protrudes itself through primitive rocks, new red sandstone, and Alpine limestones, both in the form of small dykes and irregular masses: the latter swell into mountains of great elevation at the upper extremity of the Val di Fassa, above Vigo; and in the Sieger Alp close adjoining. They abound in well crystallized minerals, chiefly of the zeolite family, which bear a strong resemblance to those of the neighbourhood of Glasgow. In both countries also the rock itself comes much into contact with, and cuts through strata of, the new red sandstone formation. A similar mass of trap occurs also cutting the Alpine limestone of Monte Baldo on the Lago di Garda, where it is remarkable for containing veins and nodules of the green earth of Verona, a substance which probably derives its colour, if not origin, from the decomposition of pyroxene.

Not far south from Fassa, on the border of the plain of Lombardy, is a still more extensive formation of trap, which occupies large tracts in the Vicentino, the Monte Berici, and Euganean Hills.

In these districts, basaltic dykes cut through rocks of all ages, from the mica slate of Recoaro to the Calcaire Grossier of Monte Bolca and the Monte Berici; and amorphous masses of trap protrude themselves into and through these same formations, so as to appear in different points both to lie under and over, and to alternate with them all. In the Euganean Hills, the trap has been said to contain marine shells; and hence an argument has been taken against its igneous origin. But these remains occur only in a species of basaltic tuf, or regenerated trap being a conglomerate rock composed of minute fragments of trap, mixed with marine shells of the same character with those that fill the strata of Calcaire Grossier with which these trap conglomerates alternate. Similar shells are found on the south of Turin at the base of the Sub-Apennines, in a breccia

composed of fragments of serpentine; and in Hungary also in an analogous breccia composed of fragments of volcanic rocks.

Although no trap rocks occur between the central Alpine Ridge and great Valley of the Danube, yet small portions of it occur on the north border of this valley at Hoent Wyl, near Schaffhausen, and again at Urach, in the Rouh Alp, on the north-west of Ulm.

Its extent a little further north in Saxony and Bohemia is too notorious to require mention.

JURA CHAIN.

The general history of the Jura Chain may be stated more briefly than that of the Alps, from which it is separated only by the great valley of Switzerland, extending from Geneva to Constance; and being in fact only the continuation of the upper extremity of the Valley of the Danube, and like it composed almost exclusively of strata of tertiary formation.

The Jura Chain runs in a line parallel to that of the Alps, and of this great valley from Nantua, on the NE of Lyons, to Neuchâtel and Schaffhausen; from whence its prolongation through the Rouh Alp on the south of Swabia connects it with Nuremberg, and the great calcareous masses of the centre of Germany.

Its dip is toward the Alps, plunging under the molasse and nagelfluë of the great valley above-mentioned. Its escarpment rises NW towards the primitive and transition rocks of the Black Forest, and the Vosges. Its component formations are, beginning with the lowest, new red sandstone, magnesian limestone, lias, and several varieties of oolite; on these latter are dispersed some irregular patches of freshwater formation and lignite. Its most obvious and most abundant rocks are beds of oolite, resembling that of the Cotswold Hills and neighbourhood of Bath; and the term Jura limestone has been applied most usually, and with most propriety, to this variety of its component rocks, particularly in the case of the oolites of France. But in Germany, many rocks which belong to the magnesian limestone formation have been confounded with the true oolite from the circumstance of their analogues having been observed in the Jura, and the Jura being erroneously considered to contain but one formation. The upper beds of the Jura Chain lying above its oolite assume the ordinary compact form of younger Alpine limestone, as may be seen at Schaffhausen; and confirm the opinion that the oolite formation is a component portion of the younger Alpine limestone.

THREE GREAT VALLEYS BORDERING ON THE ALPS.

1. *Valley of Switzerland.*

The position of the Great Valley of Switzerland between the Alps and Jura Chain, and parallel to both of them, has been

462 *Prof. Buckland on the Structure of the Alps, [June,*
 already described. It is composed exclusively of tertiary
 strata; i. e. magellue, molasse, and freshwater formations.
 These probably rest (on their NW side at least) on the prolon-
 gation of the upper beds of the Jura Chain dipping towards the
 Alps.

2. Valley of the Danube.

The same Tertiary formations extend on from Switzerland
 into the Valley of the Danube, along which they continue east-
 ward, with two partial interruptions near Vienna, into the great
 plains of Hungary and Transylvania.

The south frontier of this Valley from Switzerland to Vienna
 is formed by the Alps. Its north border, by the continuation of
 the Jura into the Rouh Alp, and afterwards by the granitic rocks
 which are connected with the great primitive mass of the south
 of Bohemia, and run in a line nearly parallel to the Danube
 from Ratisbon to Lintz and Melk. It is probable that the fun-
 damental rocks of this great valley are transition and primitive,
 since the latter are seen emerging from beneath the tertiary
 formations along great part of their north frontier. While on
 their south border from Saltzburg nearly to Vienna, they repose
 immediately on transition rocks.

3. Valley of the Po.

The central portions of the Valley of the Po are so completely
 covered by gravel, that we can only form conjectures as to the
 rocks that lie immediately beneath it. But as the first strata
 that appear on the edges of this gravel are referrible to tertiary
 formations, it is probable that their prolongations stretch across
 from the base of the Alps to that of the Apennines.

Their position and character in the Subapennine Hills has
 been well described by M. Brocchi; they may be seen to great
 advantage on the south of Parma and Placentia resting on the
 transition limestone, greywacke and serpentine of the Apennines.

On the north flank, they are admirably displayed in the Subal-
 pine Hills, near Verona and Vicenza. And at the western
 extremity, they form considerable hills on the right bank of the
 Po, immediately east of Turin: the hill of Superga, near Turin,
 is a remarkable point on this formation.

*The following Tabular Arrangement of the Rocks that occur in the
 Continental Districts under Consideration, with their Equiva-
 lents in England, may assist to establish an Understanding
 between the Geologists of the Two Countries.*

ENGLISH FORMATIONS.

FORMATIONS OF THE CON- TINENT.

Alluvium.

Effect of causes now in action.
 Head of rivers, deltas, gravel of torrents.

Alluvium.

Effect of causes now in action.
 Same as in England, but on a larger
 scale.

Diluvium.

Gravel and rolled blocks, both on hills and in valleys, not produced by any causes now in action.

Gravel of the valleys of the Thames, Severn, and Humber.

Blocks of Cumberland granite in the plain of Shropshire, near Bridgenorth; and of Galway granite at Shalk on the SW of Carlisle, in Cumberland.

TERTIARY FORMATIONS.

London and Hampshire basins.

1. *Freshwater Limestone.*

Headen Cliff, Isle of Wight.

2. *London Clay.*

Highgate Hill, London.

With plants and marine fish.
Isle of Sheppy.

3. *Plastic Clay Formation.*

Clay, marl, sand, and gravel, with marine shells.

Basins of London, Hants, and Dorset.

4. *Puddingstone, of Hertfordshire.*

Druid sandstone blocks of Buckinghamshire, Wilts, and Dorset.

5. *Lignite and Glance Coal. Imperfect Wood Coal.*

Alum Bay. Isle of Wight.

Corte Clay Pits. Isle of Purbeck.

Diluvium.

Same as in England.

Superficial gravel, covering the regular Tertiary strata of the valleys of the Po, the Danube, and Geneva.

Granite Blocks on the Jura above Neuchâtel, and on the Saleve mountain near Geneva.

TERTIARY FORMATIONS.

Basin of Paris; Valleys of the Po, the Danube, and Switzerland.

1. *Calcaire d'Eau Douce.*

Basin of Paris; Fribourg, near Bernes; St. Saphorin, near Vevey; Horgen, near Zurich; Locle on the Jura; Valley of the Rhine, three miles NE of Basle. These are principally composed of marl stone, and contain beds of coal, with Freshwater shells intermixed. Oeningin, near Schaffhausen, with freshwater fish.

2. *Calcaire Grossier of Paris.*

Verona, Vicentine Hills, and Monte Berici, in the valley of the Po. Loretto, SE of Vienna, in basin of Danube. Tour de Moliere, E of Yverdun, in Switzerland. With plants and marine fish. Monte Bolca, near Verona. Solenhofen, near Pappenheim (probably). Fish of Mount Lebaun (probably).

3. *Plastic Clay Formation.*

Beds of clay, marl, sand, and gravel, with marine shells. Basin of Paris. All the edges of the plain of Lombardy; near Parma, Placenza, Asti, Turin, Vicerza. Valley of the Danube. Valley of Geneva and Constance.

4. *Nagelfluhe of Switzerland, Como, and Saltzburg.*

Puddingstone, of Rigi, near Lucerne, and of Bregentz on Lake Constance.

5. *Lignite and Glance Coal. Perfect and used for Fires.*

Monte Bolca and Arignan in the Vicentino. Fussen in Bavaria. Titmoning, Teisendorf, Miesbach, and all the coal pits in the Valley of the Danube, above Vienna. Marburg, in Styria. Leoben, in Styria.

It is not implied that the above five subdivisional parts of the Tertiary Formations maintain the same relative order of succession in England and on the Continent; most of them probably alternate, but they are all more recent than the chalk of England, France, and Italy.

SECONDARY FORMATIONS.

Chalk.

Large proportion of the SE. of England.

Chalk.

Craie of the French, encircling and forming the base of the Basin of Paris.

Younger. Alpine limestone of the Euganean Hills and Vicentine Hills, in Italy.

Fort near Lunenburg, close to the town on the side of Hamburg.

Castle of Cracow, in Poland.

Green Sand.

Large proportion of the SE of England.

Craie Inferieure of the French.

Quader Sandstein and Pläner Kalk, of Werner.

Younger Alpine limestone of Savoy, forming the summit of the High Ridge from Mount Varens in the Vale of the Arve to Diableret, in the Rhone Valley.

Oolite Formation.

Coral rag, loose, and rubly..

Jura Limestone (properly so called.)

Younger Alpine limestone of Savoy, Switzerland, and Tyrol.

Muschel Kalk, of Werner.

Coral Rag. Same organic remains in a compact matrix, and used for marble at Roche, near Vevey.

Compact beds of Com. Brush.

Compact limestone of Schaffhausen, lying above the Jura Oolite.

Buckingham marble. Yeovil marble.

Saltzburgh marble. Conte marble, of France.

Bath and Cotswold oolite.

Oolite of Jura, and Valley of the Adige.

Lias.

Pierre a gryphite of France and Jura, full of the Grypheæ Arcuata of Lemark.

Muschel Kalk, of Werner.

New Red Sandstone and Red Marl.

Bunter Sandstone and Roth Thon of Werner.

Great formation of salt and gypsum.

First and second salt, and gypsum formation of Werner.

Greywacke of Brocchi in his Val di Fassa, of Ployer in his map of Tyrol, and of Von Buch and Charpentier in their accounts of the salt formations of the Alps.

Magnesian Limestone.

Elder Alpine Limestone. Hochgebirgs Kalk Stein of Ebel, first Floetz Limestone of Werner, divisible into

Zechstein (Calcaire a Gryphite of Voigt and Schlotheim, and containing Gryphites aculeatus.)

Asche.

Rouhwacke.

Holen Kalk.

Rogenstein.

Stinkstein.

Kupferschiefer, or bituminous marl slate, with fish.

These subdivisions are well known in the Thuringer-vald, and are occasionally interspersed with salt and gypsum.

New Red Conglomerate.

Exeter encircling the base of Quantock and Mendip Hills.

New Red Porphyry of Tolgumouth, Antrim, and Kinross.

This porphyry is associated with the New Conglomerate.

English Coal Measures, Newcastle, Derbyshire, Staffordshire, and South Wales.

Mountain, or Carboniferous Limestone.

Derbyshire, Aikton Moor, Mendip, South Wales, subordinate in the Great Coal Formation usually found in its lowest regions.

Old Red Sandstone.

In its upper members composed of loose beds of red sandstone, red marl, and conglomerate.

In its lower regions passing insensibly into compact greywacke; abundant along the frontier of England and Wales.

Old Red Sandstone of Werner, Retha Telle Liegende.

Base of Thüringerwald.

Schwanden, in Glarus.
Lugano, in Italy.

Red Porphyry of Bolzen, in Tyrol, and of Chemnitz, in Saxony.

Independent Coal Formation of Werner.

None in the Alps, or basin of Po.
Potschapel, near Dresden.
Friedland, in Silesia; and near Tarnowitz, in Silesia.
Namur, Saare Broeke, St. Etienne in France.

Transition Lime of Werner, and of Omalius d'Holley.

Banks of the Meuse from Namur to Liège; is of rare occurrence on the continent.

Variety of Greywacke of Werner.

Seldom appearing on the Continent.
Occurs at Huy on the Meuse below Namur, where it lies under the mountain limestone.
The Vallorsine puddingstone is nearly of this age, but a little older.

TRANSITION FORMATION.

Transition Limestone.

Beds of limestone occurring subordinately in the Upper Region of the Greywacke Formation, Dudley, Wenlock Edge, Ludlow, Longhope, Llandilo.

Transition Limestone occurs subordinately in Greywacke.

Thin beds of it at Coblenz on the Rhine.
In Bohemia near Prague.

Tarentaise, in Savoy.

Banks of Rhine below Coire in Swiss S of Werfen in Salzburg.

Greywacks.

Passing into fine greywacke slate at one extremity, and into conglomerate at the other.

Mountains of North Wales.

Greywacks.

Same as in England.

Abundant on the Continent.

Tarentaise, in Savoy.
Matt, in Glarus.

White quarries of Pithys.

Slatt of Tittagel, in Cornwall, and top of Snowdon, in Wales, containing marine shells (*Terebra tulites*?)

Slate of Llandridod, near Bala, containing trilobites.

Conglomerate of Killybegny and St. David's.

Slate of Blattenburg, in Glarus containing fish and tortoises.

Slate of Angers, in France; containing trilobites.

Conglomerate of Vallorsine.

Primitive Rocks.

It may be useful to add to this table of geological equivalents a list of those alpine formations which most nearly resemble each other, and which it is nevertheless very important to distinguish; e. g. there are in the alpine districts four varieties of conglomerate, four of gypsum, and five of dolomite.

Conglomerates, 4.

1. The most ancient of these conglomerates is that of the petit St. Bernard, the Vallorsine, and Tarentaise, being a true transition rock, and containing rolled fragments of granite, mica slate, gneiss, quartz rock, and primitive limestone.

2. The next in point of age is that of Schwanden and Mells near Glarus, and Mount Neisen on Lake Thun, being of the same era with the old red sandstone (rothe todte liegende) of Werner, and new red conglomerate of England.

3. The third and most abundant conglomerate is that which, under the name of nagelfluë and Rigi puddingstone, extends along the line of junction of the Great Swiss Valley with the alpine limestones from Vevey on the Lake of Geneva to Bregentz on the Lake of Constance, and thence continues onwards along the edge of the plain of Bavaria towards Salzburg. This is the most recent of the stratified rocks of this district, and is nearly of the same age with the Hertfordshire puddingstone of England.

4. The fourth, which is also called nagelfluë, consists of agglutinated masses of diluvian gravel, composed chiefly of pebbles of alpine limestone, and not to be distinguished but by the circumstances of its position from No. 3. It is usually found in the valleys, and in irregular patches on the lower hills, while No. 3 forms a chain of mountains from 3000 to 4000 feet high, which is continuous through nearly the whole of Switzerland. No. 4 abounds in all the diluvium of Switzerland, Tyrol, and Italy, when the pebbles are calcareous. It should seem these pebbles have supplied the cement by which they are held together, as the gravel is usually loose when composed of any other substance than limestone.

Gypsum Formations, 4.

In the same districts we have certainly three, probably four, formations of gypsum.

1. *Primitive.* 2. *Transition.* 3. *Secondary.* 4. *Tertiary.*

1. *Primitive.*—Existing in small quantities (if at all) in the Alps. Brochant and D'Aubuisson doubt whether there be here any true primitive gypsum, and are inclined to class that which has been considered primitive among the transition series.

2. *Transition Gypsum.*—Much of this occurs among the transition rocks of the Tarentaise described by Brochant. It may

be seen also by the road side at Charmey, between St. Michel and the Hospital of Mount Cenis, and also at the Hospital of Mount Cenis.

3. *Secondary Gypsum*.—Of the same age with the magnesian limestone, and new red sandstone formation of England, this is usually miscalled *transition gypsum* by most writers on the Alps. It occurs at Bex and Leisigen in Switzerland; in the salt mines of Tyrol and Salzburg; at Michell, 10 miles north of Trent; and Lovinio, near Menagio on the Lake of Como.

4. *Fibrous Gypsum*.—Of tertiary formation, of the same age with the Paris beds occurs in the molasse of Switzerland, near Yverdon; in Argovie, near Soloure; at Boudry, near Neufchatel; and St. Julien, near Geneva.

Dolomite, Five Kinds.

In the alpine regions there occur also five formations of dolomite.

1. *Primitive*.—The primitive limestone of the central Alps often passes into the state of dolomite, of which a good example may be seen at the pass of Mount Brenner between Botzen and Inspruck. It is here compact, and interspersed with flakes of talc, of a delicate green colour. The primitive limestone also which forms the matrix of the great iron works of Eisentertz, in Styria, is in the state of dolomite. This stratum is of great importance and extent in the Alps, and may be traced by its sparry iron ore from Lake Como to Eisenertz, and thence onwards into Hungary.

2. *Transition*.—I did not find dolomite in the few spots in which I had opportunity to see the transition limestone of the Alps; but as this formation abounds with magnesia in England, Russia, and North America, it is probable that it requires only investigation to find it also in the Alps.

3. *Elder Alpine Limestone*.—Dolomite prevails in no alpine formation so much as in this, which is equivalent to the grand magnesian limestone of England: it may be usually recognised by its pearly glimmering lustre. The soft powder that fills the cells of the roughwacke and hollen kalkstein is much charged with magnesia: as are also the strata of yellow sandy limestone that lie in the new red sandstone of the Vale of the Adige above Trent.

In England, much dolomite occurs also in the mountain limestone.

4. *Younger Alpine Limestone*.—Beds of dolomite minutely crystalline, and of pearly glimmering aspect, abound in the oolite formation in the Valley of the Adige below Trent, and also in the hills on the west of Monte Bolca. In England, magnesia has been found in the oolite formation at Minching Hampton, near Cheltenham. It occurs also in the chalk of France.

5. Tertiary Formations.—The calcaire grossier of the hills that overhang the town of Verona, and of many parts of the Vicentine Hills, passes into dolomite. The loose calcareous sandy beds that alternate with this calcareous rock also contain magnesia. Marine shells in high preservation are found both in the solid and loose varieties.

It may be useful to repeat concisely, what has already been stated more at large, that the following terms are applied indiscriminately by many writers on the Continent to rocks which ought to be kept distinct; viz. greywacke to beds of the new red sandstone formation, as well as to the true transition rocks.

Transition limestone, to the younger alpine limestone, or English magnesian limestone formation, as well as to true transition limestone.

Transition gypsum, to the saliferous gypsum of the new red sandstone and magnesian limestone formation, as well as to that which accompanies true greywacke.

Pierre à gryphite to lias. Calcaire à gryphite to magnesian limestone. Jura limestone to oolite, lias, and magnesian limestone. Nagelfluë to agglutinated gravel, Rigi puddingstone, and new red sandstone conglomerate.

ARTICLE X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

May 3.—Observations on the Variation of Local Heat made among the Garrow Hills, by D. Scott, Esq.

On some Subterraneous Trees discovered near Mundslay, by Lieut. Miles, R.N.

On the Enlargement of the Glands of the Neck, by J. Howship, Esq.

May 10.—Some Remarks on Meteorology, by Luke Howard, Esq.

Observations on the Solar Eclipse of Sept. 7, 1820, by Mr. C. Rumker.

May 17.—On the Anatomy of Parts of the Globe of the Eye, by A. Jacob, M.D.

May 24.—On the Absolute Zero, by Mr. Herspath.

ARTICLE XI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS
CONNECTED WITH SCIENCE.I. *Composition of Rhubarb.*

Mr. Brande has lately analyzed the root of the *Rheum Palmatum*. The results of the destructive distillation of rhubarb are stated as follow:

Water	10.0
Empyreumatic oil, gallic acid, and water, formed	49.0
Charcoal	34.5
Phosphate of lime.....	2.0
Carbonate of lime.....	4.2
Loss.....	0.3
	<hr/>
	100.0

The component parts of rhubarb are stated to be as follow:

Water	8.2
Gum	31.0
Resin	10.0
Extract, tan, and gallic acid	25.0
Phosphate of lime.....	2.0
Mulate of lime	6.5
Woody fibre.....	16.3
	<hr/>
	100.0

II. *Rocks of Mont Blanc.*

Owing to an accident, these rocks were not quite correctly noticed in Capt. Undrell's communication.

The very highest rock is highly crystalline hornblende and steatite; the latter might be mistaken for compact felspar, but it yields to pressure and the knife. Another specimen which is laminated seems to consist of greyish and yellowish steatite, imbedding quartz crystals, and having brown mica between the laminæ. This specimen might be readily mistaken for fine-grained gneiss. The rock called Petit Mulet, the specimen from which was mistaken for that from the summit, is the protogène of Prof. Jurine, consisting of quartz, felspar, and steatite.

III. *Granulation of Copper.*

The following singular circumstance was communicated to me by Mr. W. Keates, of the Cheadle Copper Works.

"I send you some globules of copper, quite hollow, and so light as to swim on water; the history of which is as follows: One of our refining furnaces contained about 20 cwt. of melted copper, which was to be laded into blocks; but the refining process had not been carried far enough, so that when the men came to lade it out into the moulds, they found it to be impracticable, in consequence of its emitting such

a great quantity of sulphurous acid vapour. They were, therefore, obliged to put it into a cistern of water to granulate it, but by this operation, instead of the copper assuming the form of solid grains, the whole of it became in the form sent to you, and floated upon the water like so many corks. What is the most probable explanation of this phenomenon? One of our refining men, during 40 years' experience in the business, has never seen any thing similar."

To the above account, I have only to add, that the globules of copper sent to me, although extremely light, had lost their property of floating in water, but they floated in sulphuric acid. I do not venture to offer any explanation of the phenomenon.—*Ed.*

IV. *Analysis of Indian Corn.*

Dr. Gorham, of Harvard University, Cambridge, U. S. has analyzed Indian corn. It appears to contain a peculiar vegetable substance, which the discoverer has called Zeine. The results of the analysis are as follow:

	Common state.	Dry state.
Water	9.0	
Starch	77.0	84.599
Zeine	3.0	3.296
Albumen	2.5	2.747
Gummy matter	1.75	1.922
Saccharine matter	1.45	1.593
Extractive matter	0.8	0.879
Cuticle and ligneous fibre	3.0	3.296
Phosp. carb. sul. of lime and loss	1.5	1.648
	<hr/> 100.0	<hr/> 99.980

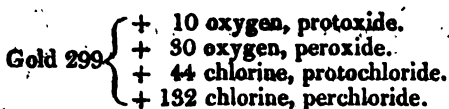
(Institution Journal.)

V. *On the Iodide, Oxides, and Chlorides of Gold.*

According to M. Pelletier, who first obtained the iodide, gold is not acted upon either by iodine or hydriodic acid; but the hydriodic acid containing iodine easily dissolves gold, and especially when a little nitric acid is added; the iodide of gold then formed is a brilliant yellow, and apparently a crystalline powder. Iodide of gold may also be procured by causing hydriodic acid to act upon the oxide of gold, or by adding hydriodate of potash to chloride of gold. The properties of the iodide of gold are, that it is insoluble in cold water, and very sparingly soluble in hot; when put into concentrated and boiling nitric, muriatic, or sulphuric acid, it is decomposed, the iodine being evolved, and the gold dissolved. It is also decomposed at a temperature of about 300° of Fahr. and by the alkalies in solution. It is stated to be composed of

Iodine	34
Gold	66
	<hr/> 100

The composition of the oxides and chlorides of gold is stated as follows:



M. Pelletier draws the following conclusions from the various experiments which he has made on this metal.

1. Gold ought to be considered as an electro-negative metal, *i. e.* as a metal forming oxides, which tend rather to act as acids than as bases.

2. The oxides of gold cannot form true salifiable compounds with the acids.

3. The peroxide of gold will unite to the alkalies and other metallic oxides, forming combinations which possess peculiar properties.

4. Gold in solution in aqua regia is in the state of perchloride, and the supposed triple salts of gold are only mixtures in which the gold is still in the state of perchloride.

5. Gold unites to iodine, forming a compound of which the proportions are constant, and easily determinable.

6. According to the proportions of the iodide of gold, those of the oxides and chlorides may also be ascertained as given in the *Mémoire*.

7. The vegetable acids and salts have different actions on the chlorides and oxides of gold. Amongst them may be distinguished the oxalic acid and the oxalates, because their action is very peculiar, and supports the opinion of M. Dulong on the oxalic acid.—(*Annales de Chimie et de Physique*.)

ARTICLE XII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

The Parent's Medical and Surgical Assistant, intended for the use of the heads of families, parochial clergymen, and others, by Thomas Ayre Bromhead, MB. Christ's College, Cambridge.

Dr. Paris and John S. M. Fonblanque, Esq. Barrister at Law, have, in considerable forwardness, a work, to be comprised in one volume, 8vo. and entitled "Medical Jurisprudence." It will comprehend medical, chemical, anatomical, and surgical investigations, applicable to forensic practice, for the instruction and guidance of coroners, magistrates, counsel, and medical witnesses, with a copious appendix of statutes, cases, and decisions.

Mr. Gideon Mantell's *Outlines of the Geology of the South-eastern Division of Sussex*, will soon be published in royal 4to. with numerous engravings.

A Treatise on Scrofula, its Nature, Treatment, and Effects; also, the alteration produced by it in the structure of all the different parts of the body, with special reference to its connexion with spinal curvature, diseases of the joints, affections of the glands. To which is added, an Account of the Ophthalmia, so long prevalent in Christ's Hospital. By E. A. Lloyd, RCS. &c. &c. in one volume, 8vo. This Work obtained the Jacksonian Prize in 1818.

Dr. Dickinson has in the press, *The Medical Student's Vade Mecum*; being a work in the form of question and answer, comprising anatomy, physiology, botany, and pharmacy, &c. To which will be added, an abridged and correct Explanation of the Chemical Decompositions, intended principally for gentlemen previous to their examinations at the Surgeons' and Apothecaries' Hall.

JUST PUBLISHED.

Flora Scotica, or a Description of Scottish Plants, arranged both according to the Artificial and Natural Methods; in two Parts. By W. Jackson Hooker, L.I.D. F.R.A. F.L.S. &c. 8vo. 14s.

A Treatise on the *Hydrocephalus Acutus*, by Leopold Anthony Golis. Translated from the German, by Robert Gooch, MD. 8vo. 8s.

Observations on the Derangement of the Digestive Organs, and some Views of their Connexion with Local Complaints. By W. Law, Fellow of the Royal College of Surgeons, Edinburgh. 8vo. 6s.

A Toxicological Chart, exhibiting at one View the Symptoms, Treatment, and Modes of detecting the various Poisons, Mineral, Vegetable, and Animal, according to the latest Experiments. By W. Stowe, RCS. London. 1s. 6d.

A Treatise on Indigestion and its Consequences, called Nervous and Bilious Complaints, with Observations on the Organic Diseases, in which they sometimes terminate. By A. P. Wilson Philip, MD. FRS; &c. 8vo. 9s.

ARTICLE XIII.

NEW PATENTS.

Stephen Wilson, Esq. of Streatham, Surrey, for improvements in machinery for weaving figured goods; partly communicated to him by a foreigner.—March 8, 1821.

Henry Browne, of Derby, chemist, for an improvement in the construction of boilers, whereby a saving in fuel is effected, and smoke rapidly consumed.—March 16.

Ilario Pellafines, of Earl's-court, Middlesex, for certain new and improved machinery and methods for breaking, bleaching, preparing, manufacturing, and spinning, into thread or yarn, flax, hemp, and other productions and substances of the like nature, capable of being manufactured into thread or yarn.—March 27.

William Southwell, of Gresse street, Rathbone-place; for certain improvements on cabinet piano-fortes.—April 5.

James Goodman, of Northampton, for an improvement on stirrups.—April 5.

Henry Goldfinch, of Hythe, Lieutenant-Colonel in the Royal Engineers, for an improvement in the formation of horse-shoes.—April 5.

William Annesley, of Belfast, architect, for certain improvements in the construction of ships, boats, and other vessels.—April 6.

William Chapman, of Newcastle-upon-Tyne, civil engineer, for a method or methods of transferring the loadings of lighters and barges into ships or vessels, or from ships or vessels into lighters and barges.—April 12.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1891.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Humid. 9 a.m.
		Max.	Min.	Max.	Min.			
4th Mon. April								
1	W	29.60	29.34	51	48	—	17	63
2	W	29.34	29.26	58	38	—	25	72
3	W	29.33	29.31	51	37	—	01	62
4	N W	29.48	29.30	55	34	—	—	64
5	N W	30.07	29.48	52	31	—	—	60
6	N W	30.07	30.03	49	42	55	10	56
7	N W	30.14	30.03	59	48	—	—	92
8	W	30.14	30.05	67	43	—	—	59
9	S W	30.05	29.86	65	44	32	—	81
10	N W	29.86	29.72	64	47	—	—	68
11	S W	29.72	29.46	61	41	—	23	67
12	W	29.46	29.38	54	32	—	—	60
13	W	29.65	29.47	54	36	—	02	62
14	S	29.53	29.36	51	37	52	12	63
15	S W	29.54	29.48	54	30	—	03	68
16	Var.	29.48	29.43	59	27	—	—	61
17	N W	29.66	29.48	58	38	—	—	58
18	N W	29.83	29.66	51	40	—	06	63
19	S W	29.74	29.61	57	48	40	36	70
20	N W	29.80	29.61	65	42	—	12	79
21	N	30.01	29.80	59	42	—	—	81
22	N E	30.02	29.62	59	45	—	—	73
23	E	29.62	29.45	70	50	30	04	81
24	S W	29.67	29.62	70	42	—	—	68
25	E	29.75	29.67	74	44	—	—	64
26	E	29.75	29.71	78	48	—	—	63
27	W	29.84	29.75	67	40	57	—	69
28	N	29.86	29.84	71	43	—	—	61
29	N	30.05	29.86	63	47	—	—	63
30	N E	30.08	30.05	51	45	25	01	67
		30.14	29.26	78	27	2.91	1.52	92—56

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fourth Month.—1. Fine: rain at night. 2. Showery: windy. 3. Squalls, with hail and Nimbi. 4, 5. Fine. 6, 7. Cloudy. 8. Very fine: thermometer 63° at half-past nine. 10. Cloudy: some appearance of thunder, p.m.: lunar halo. 11. Cloudy: windy. 12. Showery: gusty: at Tottenham a heavy hail-storm. 13. Slight showers: gusty. 14. Showery: windy. 15. Ditto: at Tottenham thunder was twice heard to the N, there being at the time many large Nimbi, and the first swallow made its appearance. 16. Fine: a hoar-frost in the morning. 17. Hoar-frost: thundered twice at half-past four, p.m. 18. Showery: frequent rainbows during the afternoon; one was observed with two complimentary bows at some distance beneath it. 19. Showery: boisterous night. 20. Slight showers during the day: very frequent lightning in the evening: a thunder-storm about seven, p.m.: the lightning extremely vivid, and nearly continuous from SW to SE, and much forked: some flashes descending perpendicularly to the earth. 21. Cloudy: clear night: swallows now numerous. 22. Very fine morning. 23. Fine: some lightning at night. 24. Very fine. 25. Ditto. 26. Sultry day: incessant lightning in the evening, which continued nearly all night in every quarter of the horizon, and very distant.

RESULTS.

Winds: N, 3; NE, 2; E, 3; S, 1; SW, 5; W, 7; NW, 8; Var. 1.

Barometer: Mean height

For the month.....	29.697 inches.
For the lunar period, ending the 25th.....	29.615
For 14 days, ending the 1st (moon south).....	29.521
For 13 days, ending the 14th (moon north).....	29.675
For 14 days, ending the 28th (moon south).....	29.689

Thermometer: Mean height

For the month.....	40.450°
For the lunar period.....	48.400
For 30 days, the sun in Aries.	45.633

Evaporation..... 2.91 in.

Rain..... 1.52

Mean of hygrometer..... 67°

Laboratory, Stratford, Fifth Month, 21, 1821.

R. HOWARD.

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